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## *ON THE DISTRIBUTION OF MARINE MAMMALS.**

### I. INTRODUCTORY REMARKS.

Most of the recent writers on Geographical Distribution have confined their attention to terrestrial mammals, or, at any rate, have but casually alluded to the marine groups of that class. On the present occasion I wish to call your attention to some of the principal facts connected with the distribution, over the world's surface, of the marine, or aquatic, members of the class of mammals.

Aquatic mammals, which pass their lives entirely, or, for the greater part, in the water are, of course, subject to very different laws of distribution from those of the terrestrial forms. As regards aquatic mammals, land is, of course, an impassible barrier to their extension, and, subject to restrictions in certain cases, water offers them a free passage. Just the opposite is the case with the terrestrial mammals, to which, in most cases, land offers a free pas-

* Read before the Zoological Society of London on March 16th.

sage, while seas and rivers restrain the extension of their ranges.

The groups of aquatic mammals that are represented on the earth's surface at the present time are three in number, viz.: (1) the suborder of the Carnivora, containing the Seals and their allies, generally called the Pinnipedia, which are semi-aquatic; (2) the Sirenia, which are mainly aquatic, and (3) the Cetacea, which never leave the water, and are wholly aquatic. We will consider briefly the principal representatives of these three groups, following nearly the arrangement of them employed in Flower and Lydekker's 'Mammals living and extinct.'

### II. DISTRIBUTION OF PINNIPEDS.

The Pinnipeds, which I will take first, comprise three distinct families—the Otariidæ, the Trichechidæ, and the Phocidæ. Beginning with the *Otariidæ*, or Eared Seals, commonly known as Sea-lions and Sea-bears, we find the greater number of the species confined to the South Polar Ocean, where they pass most of their time at sea, but, as is well known, resort to the land at certain seasons for breeding purposes. In the Atlantic Ocean, so far as I know, the Eared Seals have never been ascertained to occur further north than the estuary of the La Plata on the American coast and the vicinity of the Cape on the African coast. But in the Pacific, on the contrary, three distinct species of *Otaria* are found all over the Arctic portion of that ocean, and there are well founded traditions of Eared Seals having been formerly met with in the Galapagos, while they still occur on the coast of Peru and Chili. I think, therefore, we may assume that *Otaria* was originally an Anarctic form, but has traveled northwards along the west American coast and is now firmly established in the North Pacific. In a parallel way in the class of birds, the Albatrosses (*Diomedea*), which are essentially a group of the Antarctic Seas, are represented by three distinct species in the North Pacific.

The second family of the marine Carnivora, on the other hand, the Walruses (*Trichechidæ*), are entirely Arctic in their distribution; one species (*Trichechus rosmarus*) being peculiar to the North Atlantic, while a second nearly allied species (*T. obesus*) takes its place in the Northern Pacific.

The third family of Pinnipeds is more numerous and varied, both in genera and species, than the two preceding and has a more extended range. The Seals, *Phocidæ*, embracing about nine different generic forms, are most numerous in the Arctic and Antarctic seas, but are also feebly represented in some intermediate localities. Beginning with the North Atlantic, we find several species of *Phoca* inhabiting various parts of this area, and the Gray Seal (*Halichœrus*) and the Bladder-Seal (*Cystophora*) exclusively confined to it. In the North Pacific all the four true Seals belong to the genus *Phoca*, and three of them are identical with the North Atlantic species, but when we descend as far south as the Gulf of California on the American coast we meet with a species of Sea-elephant (*Macrorhinus*) which, like *Otaria*, has no doubt penetrated up here thus far from its ancestral abode in the Antarctic Ocean.

Returning to the central Atlantic we find two species of seals inhabiting these waters, both belonging to the same genus *Monachus*. One of these (*M. albiventer*) inhabits the Mediterranean and the adjoining coasts of the Atlantic, while the other (*M. tropicalis*) is in these days restricted to some of the smaller and less known islands of the West Indies.

The *Phocidæ* of the Antarctic Ocean all belong to genera distinct from the Arctic forms and more nearly allied to *Monachus*, the seal of the mid-Atlantic. They are of

four species belonging to as many genera: *Ogmorhinus*, *Lobodon*, *Leptonychotes* and *Ommatophoca*. Besides these the sea-elephant of the whalers (*Macrorhinus*) is essentially an Antarctic form, though now nearly extinct there, after long persecution by man. But, as already noted, it extends, or has in former days extended, far up the west coast of America, and is still occasionally found on Santa Barbara Island, on the coast of California.

### III. DISTRIBUTION OF SIRENIANS.

Only two forms of Sirenians are at the present time existing on the earth's surface —the Manatee (*Manatus*) and Dugong (*Halicore*)—each representing a distinct family of the Order. The Manatee is an inhabitant of the coasts and estuaries of both sides of the middle Atlantic Ocean—one species (*Manatus senegalensis*) occurring on the African shores, and another (*M. americanus*) on the South American coast and in the Antilles. A third species (*M. inunguis*), so far as we know at present, is found only in fresh water high up the Amazons.

The Dugong (*Halicore*) is distributed from East Africa, along the shores of the Indian Ocean and its islands, to North Australia. Three species of this genus have been established—*Halicore tabernaculi* from the Red Sea, *H. dugong* from the Indian Ocean, and *H. australis* from Australia; but it is doubtful how far these forms are actually distinguishable.

Besides *Manatus* and *Halicore*, a third quite distinct form of Sirenian was formerly an inhabitant of the North Pacific. This was Steller's Sea-cow (*Rhytina stelleri*), by far the largest animal of the group, which was exterminated by human agency about 1768. Fortunately recent researches in Bering's Island have been successful in supplying specimens of its skeleton for our principal museums, and Steller, its discoverer, left to posterity a good account of its habits and anatomy.

### IV. DISTRIBUTION OF CETACEANS.

Adopting the recognized division of the Cetaceans into two Suborders, Mystacoceti and Odontoceti, according as to whether their mouths are furnished with baleen ('whale-bone') or teeth, we will first consider the True or Whale-bone Whales, which consist of a single family, Balænidæ, usually divided into five genera: *Balæna*, *Neobalæna*, *Rhachianectes*, *Megaptera* and *Balænoptera*. Of these, *Balæna*, *Megaptera* and *Balænoptera* are almost cosmopolitan—species of them, whether distinct or not is at present more or less uncertain, being met with in nearly every part of the ocean. But *Rhachianectes* has, as yet, been ascertained to occur only in the Northern Pacific, and *Neobalæna* in the South Polar Ocean, so that we have in these cases two well-marked local types to deal with.

The Toothed Wales (Odontoceti) are more diversified than the preceding group, and are usually held to embrace at least four existing families, besides several extinct forms. The first family, containing the Physeteridæ, or Sperm-Whales, consists of at least six genera (*Physeter*, *Cogia*, *Hyperoodon*, *Ziphius*, *Mesoplodon* and *Berardius*). *Physeter* and *Cogia* are inhabitants of the whole oceanic area between the tropics, extending in certain localities some way beyond them. *Hyperoodon* is confined to the North Atlantic. *Ziphius* has an extensive range, and has been found in nearly every part of the ocean. *Mesoplodon* is also widely distributed, but is apparently more abundant in the Southern Hemisphere. *Berardius*, however, so far as we know at present, is restricted to the South Polar Ocean.

The Third family of Toothed Whales contains only the *Platanistidæ*, or fresh-water Dolphins, which although, in some cases at the present day entirely fluviatile, must

necessarily have all descended from what were originally oceanic forms. The three known genera are *Platanista* of the Ganges and Indus, *Inia* of the river Amazon, and *Pontoporia* of the river La Plata; the last form making a connecting link between the two preceding genera and the marine Dolphins.

The fourth family of Toothed Whales, containing the Dolphins, *Delphinidæ*, is very numerous in species and embraces at least fifteen or sixteen genera. But in spite of the efforts of Mr. True, who has recently given us an excellent summary of our present knowledge of them,* both the genera and species of *Delphinidæ* are still so imperfectly understood that I cannot say much about their geographical distribution. Most of the forms appear to be very widely distributed, but it may be said generally that Dolphins are most abundant in the intertropical seas and less plentiful both to the north and south of them.

There are, however, two forms that are exclusively inhabitants of the North Atlantic. These are the very remarkable Narwhal (*Monodon*), in which the male is furnished with a single enormous horn-like tusk, and the Beluga, or White Whale (*Delphinapterus*), closely allied to the Narwhal in many points of its general structure. These may be looked upon as quite isolated forms characteristic of the Arctic portion of the Atlantic, but not known in the Pacific.

### V. DIVISION OF THE MARINE AREA OF THE GLOBE INTO SEA-REGIONS.

From what has been already said, it will be evident that although many of the Marine Mammals have a wide distribution, others are very definitely localized; and a study of the latter will, I think, enable us to divide the oceanic portion of the globe into six Sea-regions, corresponding to a certain extent with the six Land-regions into which I proposed to separate the terrestrial portion of the globe in 1874, and which were subsequently adopted by Mr. Wallace in his standard work on the Geographical Distribution of Animals. I propose to call these Sea-regions:

(1) The *North-Atlantic Sea-region* or *Arctatlantis* (ἄρκτος and ἀτλαντίς = the daughter of Atlas), consisting of the northern portion of the Atlantic down to about 40° N. lat.

(2) The *Mid-Atlantic Sea-region* or *Mesatlantis* (μέσος and ἀτλαντίς), consisting of the middle portion of the Atlantic down to about the Tropic of Capricorn.

(3) The *Indian Sea-region* or *Indopelagia* (ἰνδός and πέλαγος), containing the Indian Ocean down to about the same degree of S. lat., and extending from the coast of Africa on the west to Australia and the great Oriental islands on the east.

(4) The *North Pacific Sea-region* or *Arctirenia* (ἄρκτος and εἰρήνη=pax), containing the northern portion of the Pacific Ocean down to about the Tropic of Cancer.

(5) The *Mid-Pacific Sea-region* or *Mesirenia* (μέσος and εἰρήνη) containing the inter-tropical portion of the Pacific Ocean; and finally

(6) The *Southern Sea-region* or *Notopelagia* (νότος and πέλαγος), containing the whole of the South Polar Ocean all around the globe south of the above mentioned limits.

We will now proceed to consider shortly the characteristic Mammals of these six Sea-regions.

### VI. THE NORTH ATLANTIC SEA-REGION, OR ARCTATLANTIS.

Amongst the Pinnipeds, two well-marked generic forms, the Gray Seal (*Halichœrus*) and the Bladder-Seal (*Cystophora*) are exclusively confined to Arctatlantis. The True Seals (*Phoca*) and the Walrus (*Trichechus*) are found in this region and in Arctirenia; and of the former genus three species (*P.*

*See 'A Review of the family Delphinidæ,' by Frederick W. True: Bull. U. S. Nat. Mus. No. 36; Washington, U. S., 1889.

*vitulini*, *P. græmlandica*, and *P. barbata*) are actually common to both these Sea-regions, while the Walruses (*Trichechus rosmarus* and *T. obesus*) of the two Sea-regions are perhaps somewhat doubtfully distinguishable. It may be easily understood how this has come to pass, because the Seals and Walrus may in the course of time, during unusually mild summers, have extended themselves along the north coast of the American continent into the Northern Pacific. But Arctirenia, as we shall presently show, is markedly distinguishable from Arctatlantis, by the presence of Eared Seals (*Otaria*), which is utterly unknown in the whole of the Atlantic area. *Otaria* is, in fact, as regards Arctatlantis what I have called, on previous occasions (see P. Z. S. 1882, p. 311), a 'lipotype' of Arctatlantis, but what I now propose to designate a 'lipomorph.'*

The Sirenians are entirely absent from the North Atlantic and constitute another lipomorph of that area.

Coming to the Whales, we find the *Mystacoceti* well represented in the North Atlantic by *Balæna*, *Megaptera* and *Bolænoptera*, but of these the two latter are almost universally distributed over the ocean, and *Balæna* recurs again in the North Pacific as well as in more southern latitudes, so that there is no genus of Whalebone Whales peculiar to Arctatlantis, although the great *Balæna mysticetus* has never been found elsewhere.

Proceeding to the *Odontoceti*, the case is different. Amongst the *Physeteridæ*, *Hyperoodon* is confined to Arctatlantis and, as already explained, two very well-marked types of the *Delphinidæ*, *Delphinapterus* and *Monodon*, are likewise exclusively denizens of the North Atlantic ocean. Arctatlantis, therefore, may be said to be well characterized by the possession of at least five genera of Marine Mammals not found elsewhere, viz., *Halichærus*, *Cystophora*, *Hyperoodon*, *Delphinapterus*, and *Monodon*.

### VII. THE MIDDLE ATLANTIC SEA-REGION, OR MESATLANTIS.

Mesatlantis has certainly not so many forms of Marine Mammals confined to its area as Arctatlantis, but there seem to be good grounds for its separation As we descend towards the tropics the true Seals (*Phocinæ*), which are constituted to live in colder water, gradually fall off in number, and in Mesatlantis are no longer met with. But in their place we find the genus *Monachus* or Monk Seal restricted to Mesatlantis, one species (*M. albiventer*) occurring in the Mediterranean and on the North African coast, and a second (*M. tropicalis*) being found in the West Indies. Mesatlantis is likewise the true home of the well-marked Sirenian genus *Manatus*, one species of which (*M. americanus*) frequents the coast of America and another (*M. senegalensis*) that of Africa.

As regards the Cetaceans we are not able to say that Mesatlantus, although well-furnished with many generic types of this Order, has one peculiar to it. We must, therefore, rest content with assigning two genera of Marine Mammals, *Monochus* and *Manatus*, as characteristic forms or topomorphs of the Sea-Mammal life of Mesatlantis.

### VIII. THE INDIAN SEA-REGION, OR INDOPELAGIA.

The Marine Carnivora, so far as we know, are entirely foreign to Indopelagia, but the

*On former occasions I have used the term 'lipotype' for a natural group which characterizes a particular locality by its *absence*. It would, however, perhaps be better to change the term to 'lipomorph,' because the type and its compounds have been generally employed in reference to the particular specimens of a species upon which original descriptions are based (*cf.* Thomas, P. Z. S. 1893, p. 241). In the same way a natural group which characterizes a particular country may be called a 'topomorph' (*τόπος locus* and *μορφή forma*). Thus in Africa *Giraffa* and *Phacochærus* would be 'topomorphs,' and *Cervus* and *Ursus* would be 'lipomorphs.'

Sirenians are well represented by the Dugong (*Halicore*), which pervades all its northern coasts from North Australia to India and the Red Sea and down the African coast to Lamu*. Whether the species of *Halicore* found at different points within this area are the same or different is still a matter of discussion, but there can be no doubt that *Halicore* is an exclusive inhabitant of Indopelagia. As regards the Whales of Indopelagia, we know that *Physeter*, *Cogia* and *Ziphius*, and numerous forms of *Delphinidæ*, occur there, but I am not aware of any Cetacean that is entirely restricted to this Sea-region.

### IX. THE NORTH PACIFIC SEA-REGION, OR ARCTIRENIA.

As was pointed out when speaking of Arctatlantis, Arctirenia has one genus of *Phocidæ* (*Phoca*) in common with the North Atlantic, and three of the species of this genus appear to be actually identical in these two Sea-regions, whilst a fourth *Phoca* (*P. fasciata*) is only found in the North Pacific. The Walrus (*Trichechus*) is again a form of Marine Mammals, common to both the great northern Sea-regions. But the feature of Pinnipedian life that absolutely distinguishes Arctirenia from Arctatlantis is the presence in the former of three (if not four) well-marked species of the Eared Seals (*Otariidæ*), which are absolutely unknown in the vast extent of the Atlantic down at least to 30° S. lat.

Arctirenia has unfortunately lost its Sirenian, Steller's Sea-Cow (*Rhytina stelleri*), the largest and finest modern representative of this formerly prevalent group, which since the days of the Pleistocene, has greatly diminished in numbers, but I think we may still treat *Rhytina* as one of the characteristic forms of the Arctirenian Sea-region. The North Pacific is also even at the present day the sole possessor of a remarkable genus of Whalebone Wales which combines the long head and elongate form of *Balænoptera* with the smooth skin of the throat and absence of the dorsal fin of *Balæna*.* This is the Gray Whale, *Rhachianectes glaucus* of Cope, which, in these days, is confined to the North Pacific, and does not range farther south than the 20th parallel in that ocean. At the same time it should be stated that indications have been discovered that a nearly allied form existed in the Atlantic in previous geological ages, though this is by no means certain. Besides *Rhachianectes* *Balæna*, *Megaptera* and *Balænoptera* are all represented in the North Pacific, and also many species of *Delphinidæ*, of which little is at present known. But *Rhytina* and *Rhachianectes* are the only genera of Marine Mammals absolutely confined to Arctirenia.

### X. THE MIDDLE PACIFIC SEA-REGION, OR MESIRENIA.

The Eared Seals, *Otaria*, must have necessarily passed through Mesirenia in their passage from south to north, though the only record of their recent presence in the central part of the Pacific is, so far as I know, the report that they were formerly found in the Galapagos. It should be stated, however, that Tschudi records the occurrence of two species of *Otaria* on the islands of the coast of Peru, and that in 1802 Humboldt met with an Eared Seal on the island of San Lorenzo, in the Bay of Callao, which is only some 12° south of the Equator.

Like *Otaria*, the Sea-elephant (*Macrorhinus*) has apparently in former ages travelled up the South American shores and established itself as far north on the coast of California as about 34° N. lat. The California Sea-elephant has been discriminated by Gill as a distinct species (*Macrorhinus angustirostris*), but its differences from

* A fine specimen of the Dugong from Lamu (on the east coast of Africa, lat. 2° 50′ S.), obtained by Mr. J. C. Haggard in 1885, is in the British Museum.

* Flower and Lydekker, Mammals, p. 241.

the southern form (*M. leoninus*) seem to be but trifling.

As regards the Cetaceans of Mesirenia, our information is at present very imperfect, and I have little to say except that species of *Megaptera*, *Balænoptera*, *Physeter*, *Cogia* and *Ziphius* certainly occur there, besides many representatives of the widely spread *Delphinidæ*.

### XI. THE SOUTHERN POLAR SEA-REGION, OR NOTOPELAGIA.

The wide ocean which surrounds the Southern Pole on every side, and extends up to 40° S. lat., seems to present, as regards its marine mammals, a nearly homogeneous fauna, which we will now briefly consider. In the first place it contains representatives of four genera of true *Phocidæ*—*Ognorhinus*, *Lobodon*, *Leptonychotes*,* and *Ommatophoca*, which are peculiar to the southern seas, and are quite distinct from all their northern representatives in the Arctic Ocean. The Sea-elephant, *Macrorhinus*, is also a denizen of Notopelagia, though, as we have already seen, it has wandered north along the South American coast far into Mesirenia.

Like *Macrorhinus*, *Otaria* also, containing the group of Eared Seals, appears to have been an Antarctic group, and the greater number of its species, although now-a-days very much reduced in numbers, are still found in the Southern Ocean. But the *Otariæ* have travelled still further north than *Macrorhinus*, and three, if not four, species, as already stated, are in these days well established inhabitants of Arctirenia.

The Sirenians are absent from Notopelagia, but Cetaceans of every kind are abundant. Besides one or more representatives of the true Whale-bone Whale (*Balæna*), Notopelagia has a smaller representative of the group (*Neobalæna*) entirely restricted to its area. It has also representatives of *Megaptera* and *Balænoptera*, though it is doubtful how far they are even specifically distinct from some of their northern representatives.

Among the Toothed Whales (*Odontoceti*) we find a large Ziphioid form, *Berardius*, restricted to the Notopelagian area, while *Ziphius* and *Mesoplodon* also occur there. The Dolphins (*Delphinidæ*) are likewise numerous and present some distinct species, but not, so far as our present knowledge extends, any generic forms that do not occur elsewhere.

But Notopelagia is sufficiently distinguished from all the five more northern Sea-regions by possessing four genera of Seals and two of Cetaceans entirely restricted to its area.

### XII. CONCLUSIONS.

It has, therefore, I think, been shown that for the Geography of Marine Mammals the Ocean may be most conveniently divided into six Sea-regions, which are as follows:

I. *Regio Arctatlantica*, characterized by its Seals (*Phocinæ*), of which two genera, *Halichœrus* and *Cystophora*, are peculiar, whilst *Phoca* is common to it and Arctirenica; by the absence of Sirenians; and by the possession of three peculiar genera of Cetaceans (*Hyperoodon*, *Delphinapterus* and *Monodon*).

II. *Regio Mesatlantica*, sole possessor of the Monk-seal, *Monachus*, amongst the Pinnipeds, and of the Sirenian genus *Manatus*.

III. *Regio Indopelagica*, characterized by the presence of the Sirenian *Halicore* and by the absence of Pinnipeds.

IV. *Regio Arctirenica*, with *Phoca*, like the Regio Arctatlantica, but having *Otaria* also; the home of the (now extinct) Sirenian *Rhytina* and of the endemic Cetacean *Rhachianectes*.

* This generic term, established by Gill in 1872, seems to take precedence of *Pœcilophoca*, proposed by Flower and Lydekker for the same type (*L. weddelli*) in 1891. *Cf.* Allen, North American Pinnipeds, p. 418.

V. *Regio Mesirenica*, without true Seals (*Phocinæ*), but having *Otaria* and *Macrorhinus* from the south; no Sirenian known.

VI. *Regio Notopolagica*, characterized by four endemic genera of *Phocidæ*, and by the presence of many *Otariæ*; without Sirenians, but with two endemic forms of Cetaceans (*Neobalæna* and *Berardius*).

In conclusion, I will call attention to some of the more remarkable points in the general distribution of the marine Mammals, and to their apparent significance.

In the first place it is evident that the Pacific has much more in common with the Notopelagian region than the Atlantic. *Otaria* and *Macrorhinus*, quite unknown in the Atlantic, extend themselves to the northern extremity of the Pacific, the former pervading that ocean up to Behring's Straits, and the latter reaching to the Californian coast. It follows that in former ages there must have been some barrier in the Atlantic which did not exist in the Pacific to stop their progress northwards. The only barrier I can imagine that would have effected this must have been a land uniting South America and Africa, across which they could not travel. Adopting this hypothesis, we have, at the same time, an explanation of the presence of the Manatee on both the American and African coasts. The Manatee could hardly live to cross the Atlantic. It is only found close to the coast, where it browses on sea-weeds and other vegetable food in shallow water. How did it travel from America to Africa (or *vice versa*), unless there were a continuous shore-line between them? The same may be said of the Monk Seal (*Monachus*), of which one species lives in the Mediterranean and on the African coast and Islands, and another in the West Indies. We can hardly believe that these creatures could easily traverse the whole Atlantic. The hypothesis of a former barrier of land between Africa and America, which we know is supported by other facts of distribution,* would alone explain the difficulty.

On the other hand, in the Pacific we find no such break between the north and south. The aquatic Mammals of Notopelagia have evidently had free access to the whole Pacific for a long period and have well availed themselves of this facility.

Again, while the great Southern Ocean exhibits a considerable uniformity of marine Mammalian life, we see the Northern waters divided into two distinctly recognizable Regions by the interposed masses of land. All these facts, with the one exception of the supposed Atlantic Barrier, would tend in favor of the now generally accepted doctrine that the principal masses of land and water are not of modern origin, but have existed mainly in their present shapes throughout all ages.

P. L. Sclater.

Zoological Society, London.

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### *FORMER EXTENSION OF CORNELL GLACIER NEAR THE SOUTHERN END OF MELVILLE BAY.*†

The initial effort of Professor Tarr's paper is to controvert the opinions recently expressed by Chamberlin and Salisbury respecting the former extension of the general glaciation of Greenland, their view being that the coast was not universally and profoundly overwhelmed by the inland ice. The observations of Professor Tarr had a range in latitude of about 5° 30′, those of Mr. Chamberlin about 17° 15′, and those of Professor Salisbury about 12°. The joint observations of Chamberlin and Salisbury covered 18° 30′, the range of their landings being about 13° 40′. These landings embraced thirteen different localities, counting the numerous landings on the border of Inglefield Gulf as one. This statement has

* *Cf.* Wallace, Geogr. Distrib. Vol. I., p. 156.

† Bull. Geol. Soc. Amer., Vol. 8, pp. 251–268, Plates XXV.–XXIX., March, 1897.

FIG. 1. "THE DEVIL'S THUMB. Elevation 2,650 feet; glaciated surface in foreground; rugged, angular topography in background on left. Transported pebbles obtained on crest of the Devil's Thumb. Photograph by J. O. Martin." [Author's legend.]

its occasion in the too insistent implication of the author of the paper under review that the observations whose validity he questions were limited to those made chiefly from a passing vessel. Of the latitude covered by the observations of Chamberlin and Salisbury about 8° 30′ lay south of the tract seen by Professor Tarr, and about 4° 30′ lay to the north of it. If the total distribution of observations be divided into three parts, 8° 30′, 5° 30′ and 4° 30′, in order from south to north, Professor Tarr's observations fall within the middle division and cover less than one-third of the whole.

It appears, therefore, that the testimony of 5° 30′ is being urged to set right the testimony of 18° 30′ in a matter of *general* conclusions. In such an attempt it would seem altogether imperative that an author urging conclusions from the minor fraction should have ascertained, with scrupulous care, whether his own observations within that fraction confirmed or contradicted the *coincident* part of those made upon the much wider tract. Singularly enough, this vital prerequisite is neglected, although an explicit statement covering this common territory had been made by Professor Salisbury (*Journal of Geology*, Vol. III., 1895, pp. 876–877).

What are the respective conclusions relative to this common tract? Professor Tarr insists upon general glaciation. Salisbury and Chamberlin believe in general glaciation with the exception of some high peaks and lee faces. Of the exceptions named by them none was visited by Professor Tarr. There is, therefore, no direct observational conflict. More than this, no observations of the one party demonstrate glaciation where the other thought it absent. The grounds for an issue are, therefore, rather tenuous. The two sets of observations are in reality rather confirmatory than conflicting. The issue has arisen from an attempt to adjudicate the whole coast by a fraction which happens to be intermediate in type, having been neither strongly subjugated by glaciation nor left conspicuously intact.

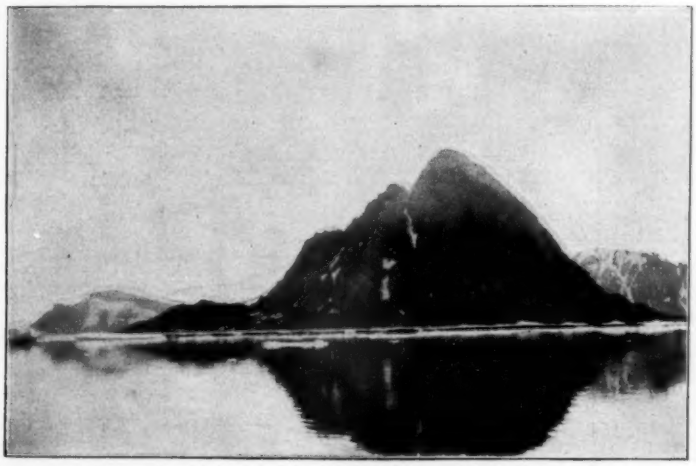

FIG. 2. Dalrymple Island—Type of unglaciated topography. (Bull. Geol. Soc. Am., Vol. VI., pp. 219; also Jour. Geol., Vol. II., 1894, p. 661.)

The author recognizes that the conclusions of Chamberlin and Salisbury are based upon evidence of two kinds: "one, the presence of a driftless area in the Inglefield Gulf region, announced by Professor Chamberlin; and a second, the angular topography of the Greenland coast, described by both." The first evidence, although vital to a *general* conclusion, is passed without discussion, because it was not seen by the author. He remarks, however, in a footnote that he "cannot let this opportunity pass without raising the query whether the topography in the neighborhood of the Greenland driftless area is not such that an area of this sort would naturally be expected. Was not the movement of the ice outward and the main stream down the Inglefield Gulf? And is not the driftless area located in the place where the high Red Cliff peninsula would naturally have clogged the ice and hence prevented its action of erosion and notable transportation?" The driftless area is a part of the same ancient peneplain as the summit of the Red Cliff peninsula (*Journal of Geology*, p. 205–6, Vol. III., 1895). It lies on the *east side of Red Cliff peninsula* (see map, p. 668, Vol. II., *Jour. Geol.*). It lies *between it and the great ice cap*. It is separated from the peninsula by the valley of Bowdoin bay, about two miles wide and 2,000 feet deep. How an isolated part of a peneplain can protect from glaciation another part of the same plain lying between it and the source of the glacial motion and several miles distant is not easily understood. The suggestion apparently sprang from the same lack of circumspection that gave birth to the main issue without adequate grounds.

The evidence drawn from topography is the main subject of discussion in the initial portion of the paper. The essential point urged in the paper and reiterated in subsequent discussion is that angularity of topography is compatible with general glaciation, and that general observations on topographic contours have little or no value in deciding the prevalence of glaciation. The most important illustrative evidence in support of this is a photographic view of 'The Devil's Thumb.' The photograph is here reproduced (Pl. 26, Vol. VIII., 1886, *Bull. Geol. Soc. Am.*). (Fig. 1.) Nothing has recently so astonished the present writer or his colleague as the presentation of this photograph as an illustration of the *absence* of topographic signs of glaciation. It is a marvel to us how any glacialist could fail to read from these contours—even from the summit contours— just that degree of glaciation which was found by the more rigorous lines of study. It is true that the lee side is angular, but this does not in any essential way confuse or obscure the dominant expression, which is that of moderate glaciation. That geologists may see how different are the contours that led Professor Salisbury and the writer to infer absence of glaciation in certain other regions, there is here reproduced the photograph of Dalrymple Island which was used as an illustration of the asperities of a typical angular topography (*Jour. of Geol.*, Vol. II., p. 661). (Fig. 2.) It must be evident to the critical observer that the two topographies belong to distinct types. The contours of Dalrymple Island clearly show the absence of any glacial softening. The contours of the so-called Devil's Thumb and of the adjacent region clearly portray a moderate measure of glacial softening. The normal asperities of a non-glacial arctic topography are gone. This topographic expression seems to the reviewer to be such as to be read with ease and accuracy even at a distance, especially after the general habit of the region has been determined by more rigorous lines of evidence. The photograph does not sustain the author in calling the topography angular in an unqualified sense; nor does it sustain him in insisting that it is impra-

ticable for observers to detect such signs of glaciation.

An additional source of confusion is introduced by naming the promonotory of the photograph 'The Devil's Thumb.' It is stated in a foot-note that "This is the Devil's Thumb as given on the Danish and British Admiralty charts. The real Devil's Thumb of the Arctic explorers is some forty or fifty miles to the north of this" (p. 254). The true Devil's Thumb is, however, sketched on the British Admiralty chart. The sketch as there given is herewith photographically reproduced (Fig. 3). It will

FIG. 3. The Devil's Thumb, as given on the British Admiralty Chart.

be seen that it does not bear even a remote resemblance to the promontory illustrated in the paper. It is to be assumed without question that the latter stands at the location designated on the charts as The Devil's Thumb, but this location is obviously an error. It does not seem to us that it justifies the transfer of the name Devil's Thumb to a new promontory to which the name has no fitness. It is especially unfortunate to introduce a second Devil's Thumb in this connection, because the true Devil's Thumb, by reason of its slenderness and angularity, has a significant bearing on the question of glacial extension, and together with Melville Monument a similar pinnacle somewhat further north has been so cited. Misapprehension has already arisen on account of this double use of the name. The slenderness and angularity of the Devil's Thumb are exaggerated in the sketch of the Admiralty chart—about as much as such objects are usually exaggerated by the impressions of the average observer. With some discount for this it gives a fair idea of this singular landmark.

If the true Devil's Thumb, Dalrymple Rock, and similar instances of angularity and asperity are taken as one type; if the contours illustrated by the paper under review be taken as a second type; if the contours of the Carey Islands (*Journal of Geology*, Vol. II., p. 662) be taken as a third type, and if the plainer topography north of Godhaab be taken as a fourth type, something of the degrees and gradations of topographical modification which the coast belt of Greenland suffered at the hands of glaciation will be indicated.

The view of the author that the peaks would remain more angular than the valleys seems perfectly valid so long as the conditions are *limited to the border of the ice cap*. Near the border the thickness of the ice in the valleys is much greater than upon the hills. The main flowage is through the valleys, and subjugation of the heights is relatively slight. On the other hand, this reasoning entirely falls to the ground when applied to profound glacial submersion, such as is implied by an advance of the 100 or 200 miles necessary to reach the heart of Baffin's Bay. Such an advance means a depth of at least 5,000 feet of ice on the peaks. In this case the difference between the depth on the heights and in the valleys is relatively much less, and three well established principles combine to emphasize the erosion of the peaks: (1) the upper portion of the ice moves faster than the lower; (2) erosion is correlated with rapidity of motion by some high power of the rate; (3) peaks yield to erosion more than embossments of the valley, because (a) the surface exposed to action is relatively greater, and (b) the attachment, and hence the resistance, of the exposed parts is relatively less. The erosion, therefore, proceeds on the crests with a facility superior to that in the

valleys by the measure of some multiplier much greater than unity. When valleys are irregular the basal retardation is still further increased and the movement of the ice is correspondingly transferred to the upper horizons. The irregularities of the coast of the region in question give this fact special application. The interpretation of the author is, therefore, quite consistent with a *limited* extension of the ice border, but quite inconsistent with profound extension. The whole of the phenomena described in the paper are precisely concordant with moderate extension. They are as precisely discordant with great extension.

The remainder of the paper consists of a description of the Cornell glacier, of the evidences and amount of former invasion, of the recent advance and retreat of the ice, and of the evidences of present retreat of the Cornell glacier. This portion embraces much valuable data, unless it is vitiated, as it probably is not, by the lack of care which marks the controversial part. It is accompanied by excellent photographs, all of which, as the writer would interpret them, show evidences of greater or less glacial modification of contour.

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## *SUGGESTIONS FOR A NEW METHOD OF DISCRIMINATING BETWEEN SPECIES AND SUBSPECIES.*

ACCORDING to present usage the rule which determines whether a particular animal or plant shall stand in our books as a species or subspecies may be stated as follows: *Forms known to intergrade, no matter how different, must be treated as subspecies and bear trinomial names; forms not known to intergrade, no matter how closely related, must be treated as full species and bear binomial names.* This principle was first distinctly formulated in the Code of Nomenclature of the American Ornithologists' Union, published in 1886. In the remarks that follow, the authors of the Code state: "The kind or quality, not the degree or quantity, of difference of one organism from another determines its fitness to be named trinomially rather than binomially. A difference, however little, that is reasonably constant, and therefore 'specific' in a proper sense, may be fully signalized by the binomial method. Another difference, however great in its extreme manifestation, that is found to lessen and disappear when specimens from large geographical areas or from contiguous faunal regions are compared is, therefore, not 'specific,' and, therefore, is to be provided for by some other method than that which formally recognizes 'species' as the ultimate factors in zoological classification. In a word, *intergradation* is the touchstone of trinomialism."

Eleven years have now elapsed since the publication af the A. O. U. Code of Nomenclature, in which the above canon and statement were first published. During this period the plan advocated has been very thoroughly tested, not only by ornithologists, but by systematists in many other departments of zoology, and also in botany. The time has come, therefore, when it should be possible to examine its practical workings, with a view to ascertaining whether or not the system is satisfactory.

In practice it has been found that only in a small percentage of cases does an author have at his command a sufficiently large series of specimens, from a sufficient number of well-selected localities, to enable him to say positively that related forms do or do not intergrade. The result of this obvious embarrassment is that authors usually exercise their individual judgment as to the *probable* existence or non-existence of intergradation, thus introducing the personal equation it was hoped to avoid. The natural result is a degree of inconsistency in the use of trinomials which has formed the subject

of much criticism, and which, under existing rules, it seems impossible to avoid. But its inconsistencies are not the only objection to the present system. From the nature of the case, increase in knowledge as to the interrelations of forms often shows that those treated as full species really intergrade, and that closely related forms supposed to intergrade really remain distinct, necessitating corresponding changes from a binomial to a trinomial, and *vice versa*. Changes of this kind may be found in the A. O. U. Check-List of North American Birds by comparing the editions thus far issued.

It will be many years, even in America, before it will be possible to say that certain forms do or do not intergrade, and until that time a fixed nomenclature will be impossible.

In view of the objections to the present system—incurable inconsistency, inevitable changes with increase of knowledge, and consequent delay in attaining a fixed nomenclature—and also in view of what to me seems the logic of the case, it would appear desirable to modify the system in the interest of consistency, stability and common sense.

In systematic zoology and botany a knowledge of the *degree of difference* between related forms is infinitely more important than a knowledge of whether or not the intermediate links connecting such forms happen to be living or extinct. It would seem, therefore, since it is is impossible for our nomenclature to tell everything we wish to know about a species, that it would serve a more useful purpose if the terms species and subspecies were so used as to indicate degree of difference, rather than the author's opinion as to the existence or non-existence of intergrades. It may be argued that 'degree of difference' is an elastic term, incapable of measurement and subject to the same personal equation that besets the present system. While this is to a certain extent true—since authors rarely see objects through the same spectacles—it is also true that individual opinion as to whether or not an observed degree of difference is worthy of specific recognition would vary within much narrower bounds than in the alternative case of hypothetical intergradation; and, further, that the change in nomenclature incident to the discovery of new facts, inevitable under the old system, would be entirely done away with.

This leads to what is, after all, the most practical consideration in connection with the proposal to be governed by *degree of differentiation rather than intergradation* in our choice of binomial and trinomial nomenclature, namely, the quantity of difference it is desirable to accept as a measure of specific distinctness. Some authors, like Mr. Lydekker in England, and Mr. Roosevelt in this country, would have us limit the number of species to types of groups, many of which are commonly regarded by naturalists as of subgeneric or even generic weight. Among the larger mammals their species are nearly always used in a superspecific sense. Thus they would have one large wolf, one small wolf, one black bear, one large brown bear, and so on, urging that the recognition of a number of related species is inconvenient, interfering with the clear and easy comprehension of the different groups. Of course this is true, but since the function of the naturalist is neither to create nor destroy species, but to recognize, describe and learn as much as he can about those which nature has established, a difficulty arises in carrying out their views of classification. It is one thing to say—without taking the trouble to find out the characters that distinguish a batch of species—what one thinks ought to be done for the easier comprehension of the science; a very different thing to arrange the animals themselves in accordance with the species which actually exist.

A good deal, of course, depends on the point of view. Mr. Lydekker as a paleontologist and compiler of excellent general works on natural history, and Mr. Roosevelt as a hunter and writer of the best accounts we have ever had of the habits of our larger mammals, find it inconvenient and annoying to be confronted by a large number of species. Still, if we examine the writings of these authors closely it becomes evident that they usually accept without complaint such species as have been currently recognized by their predecessors. This is only human nature, for are we not always more ready to challenge the announcement of new facts than to suspect those with which we have been long familiar?

In my judgment, forms which differ only slightly should rank as subspecies even if known not to intergrade, while forms which differ in definite, constant and easily recognized characters should rank as species even if known to intergrade.

In a recent article in SCIENCE, Mr. Roosevelt protests against the use of the word species where "it has entirely different weights in different cases," and cites examples of what he considers its proper use. But he forgets a host of cases in which admittedly distinct species are not separated by any such gaps as those he mentions. Mr. Roosevelt, in addition to being a good deal of a mammalogist, is something of an ornithologist, and has made contributions of value to ornithological literature. He knows, therefore, that in the eastern United States we have two species of falcons belonging to the genus *Falco*, the Sparrow Hawk and the Duck Hawk, and two species of woodpeckers belonging to the genus *Melanerpes*, the Red-headed Woodpecker and the Red-bellied Woodpecker, in both of which cases the species are separated by the kind of gaps he likes. He knows also that we have two species of thrushes, the Olive-back and Alice's, and two species of small flycatchers, Traill's and the Least, in both of which cases the species are so much alike that a trained eye is necessary to tell them apart. What will he have us do with these birds? Shall we unite the two thrushes and the two flycatchers? If not, how can he reconcile his theory to the enormous difference in weight of characters that distinguish the species of hawks and woodpeckers, contrasted with those that distinguish the thrushes and flycatches? The real difficulty is that in nature some existing species are closely related, while others are widely separated. Still, suppose for the sake of argument that we do attempt to carry out Mr. Roosevelt's suggestion to lessen the number of species by uniting some of those that are more or less closely related, and suppose we select for this purpose two groups of mammals—the bears and coyotes—against whose species he has developed such a violent aversion. If in case of the bears we try to get rid of either the Grizzly (*Ursus horribilis*), the Barren-ground (*U. richardsoni*), the Yakutat bear (*U. dalli*), or the huge Alaska Peninsula bear (*U. middendorffi*), and in the case of the coyotes we aim to abolish any one of half a dozen species, as the northeastern *Canis latrans*, the California *C. ochropus*, the Rio Grande *C. microdon* or the Mexican *C. cagottis*, we are at once confronted by the same difficulties that would beset Mr. Roosevelt were he to undertake to unite under a smaller number of specific names such birds as the Hermit, Wood, Olive-back, Bicknell's and Wilson's thrushes, or the Warbling, Red-eyed, White-eyed, Hutton's and Philadelphia vireos. These difficulties are of several kinds and involve the solution of such questions as: (1) How many and what species shall be selected as the favored ones with which the others shall be merged? (2) Which of the species to be

distributed among the chosen ones shall go to this and which to that? (3) When we have made what seems to be the best conglomeration practicable with the material at hand, how are we to frame descriptions that will cover such incongruous assemblages and distinguish them from one another? And after all (4) why should we try to unite different species under common names? It is well to remember that the book of nature is not always easy to read, and that in the great majority of cases we know nothing of the ancestry of individual species.

A prolific source of error respecting the interrelations of allied forms is the common assumption that such forms are necessarily derived from one another. In numerous instances this is not the case, their origin dating back to a common ancestor now extinct. Thus a species which in Pleistocene times had a transcontinental distribution may have given off in remote parts of its range several lines of descendants, each of which has since spread over so large an area that the resulting forms, originally widely separated, have now come to inhabit contiguous areas and as a consequence are assumed to intergrade.

Possibly the skepticism of Mr. Lydekker and Mr. Roosevelt as to the validity of the new species of mammals recently described is a result of unconsciously overlooking the wide difference in the present status of the sciences of ornithology and mammalogy. Relatively, ornithology is a finished science, while mammalogy is yet in its infancy. Birds have been studied by scores of able naturalists; mammals by comparatively few individuals. The disproportion in available material is even greater, for museums containing many thousands of bird skins rarely have more than a few hundred mammals. Until recently our museums have made no effort to secure series of mammals from extreme points in their geographic ranges, so that specimens might be placed side by side for direct comparison in order to ascertain positively—instead of assuming theoretically—what the differences really are. Even to-day no museum in the world possesses anything like an adequate series of any of our larger mammalia. In the few cases in which specimens of supposed single species have been brought together from widely separated areas it has generally been discovered that two or more species had been confounded under a single name.

In America the science of mammalogy took a long sleep after the pioneer work of Audubon, Bachman and Baird, which ended with the publication of their great works in 1854 and 1857. From this time until about ten years ago little advance was made. Then an active interest in the subject sprang up and scientific collecting really began. It is probably safe to say that during the past decade more mammals have been collected in North America alone than were previously contained in all the museums of the world. Furthermore, these specimens are not only of infinitely better quality than the old, but are accompanied by full data, uniform field measurements and perfect skulls. As a result, it is now becoming possible, for the first time in the history of the science, to bring together for actual comparison series of specimens in the different groups covering the greater part of the range of these groups in an entire continent. Is it surprising that the study of such material should result in the discovery of many new species? As a matter of fact, during the last ten years the number of species known in North America has been considerably more than doubled, and several entirely new genera have been found.

In criticising a recent paper of mine on the coyotes, Mr. Roosevelt says: "The important point is the essential likeness of all the coyotes one to the other, and their

essential difference from the big wolves with which they are associated, and which are themselves essentially like the big wolves of Europe and north Asia; and it seems to me that these facts can best be brought out by including the coyote and the wolf in one genus and treating each as a species. Then the geographical and other varieties may or may not be treated as worthy of subspecific rank according to the exigencies of the particular case."

The above remarks are based on a total misapprehension of the facts in the case and remind one of the judge who gave his decision first and tried the case afterward. As a matter of fact, two assumptions are made by Mr. Roosevelt which are widely at variance with the facts. The first is the assumed 'essential likeness of all the coyotes one to the other;' the second, the assumed 'essential difference [of the coyotes] from the big wolves.' I can show Mr. Roosevelt a series of skulls of wolves from the United States in which the great gaps are not between the big wolves and coyotes, but between two species of big wolves and two of coyotes. Thus, there is an enormous gap between the large northern coyote (*C. latrans*) and the small *C. microdon* from the lower Rio Grande, and another great gap between the big red wolf of Arizona and the big gray wolf of Wyoming. On the other hand, no such gap exists between the northern coyote and the big red wolf of Arizona, the skulls and molar teeth of these species resembling one another surprisingly. Mr. Roosevelt's third assumption, the assumed essential likeness of our big wolves to the big wolves of Europe, may be correct or incorrect according to the parts of Europe and America from which specimens are taken. The southern wolves of the two countries are too unlike to require close comparison, and even in the case of the northern forms the specific distinctness is apparent as soon as the skulls are brought together. Thus, not to mention other differences, the long muzzle and narrow forehead of the wolf of our northern plains offer a sufficient contrast to the short muzzle and broad forehead of the Scandinavian animal.

In my paper on the coyotes eleven forms were recognized, of which seven were named for the first time. All were treated binomially, but it was intimated that *pallidus* and *lestes* would probably be found to intergrade with *latrans*, and that *estor* might intergrade with *mearnsi*, leaving eight as distinct species. It was stated that the available material was insufficient to admit 'of determining which members of each group do and which do not intergrade,' for which reason it was necessary, in obedience to the rule respecting the use of specific and subspecific names given at the beginning of this article, to treat all as species. This was done reluctantly and with the conviction that the rule is illogical and should be changed. If the plan here recommended is adopted we need not care whether intergradation occurs or not, but may bring together as subspecies the closely related forms, and accept as species those more distantly connected.

In conclusion, let me appeal to museums, sportsmen and naturalists to take advantage of every opportunity, before it is too late, to secure and preserve specimens of our larger mammals from remote parts of their ranges. In Europe it is certain that many species have been exterminated through the agency of man, and in this country the process is not only about to be repeated, but has already begun. The familiar story of the vanishing buffalo is only one of many. The largest carnivorous animal of the United States, the giant grizzly of southern California, is on the verge of extinction, and it is doubtful if a museum specimen will ever be obtained. The large wolves have been exterminated over more than half the area they formerly

possessed, and no one knows what forms have disappeared and an unknown form of elk or wapiti which within the memory of our fathers—and of some men still living—inhabited the Alleghany region from North Carolina to the Adirondacks has been wiped off the face of the earth.

C. Hart Merriam.

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## THE NATIONAL ACADEMY OF SCIENCES.

### ON THE VARIATION OF LATITUDE.*

At the autumn meeting of the Academy in 1894 the author had presented the numerical theory of the motion of the pole, synthetically derived from the observations from the beginning of the history of the astronomy of precision up to that time, in its complete development, exactly as it stands to-day. Since then he had been interested to compare it with the various series of observations subsequently published, not only for the purpose of verification and improvement of the numerical values of the various constants, but also to detect any additional characteristics which these later data might make apparent. These additional investigations had individually been neither extensive nor important enough to call for separate publication; since their general result has been merely a satisfactory confirmation of the previous deductions as to the nature of the law of these motions, without furnishing material improvement of the numerical elements. But sufficient material has thus been gradually accumulated to make the present communication of some interest.

The new material to be here utilized consists of the various series of observations by Tallcott's method up to the middle of 1896, so far as published, at the following European stations, named in order of longitude: Kasan, Vienna, Prague, Berlin, Potsdam, Karlsruhe and Strassburg. In America we have Doolittle's series at Bethlehem, which was brought to a close in the summer of 1895. He is now carrying forward a new series at Philadelphia of which we may hope soon to see the results. Of the series at Columbia University, by Rees, Jacoby and Davis, begun in the spring of 1893 and still current, there have come to hand the results for the first fourteen months. It is an extremely fortunate circumstance that a portion of this series, yet unreduced, will bridge the gap in Doolittle's work rendered unavoidable by his removal from Lehigh University to the University of Pennsylvania.

The curves of latitude variation from these various series were then exhibited, and compared with the numerical theory. This comparison shows a fidelity of representation eminently satisfactory, the differences between computation and observation being practically within the range of errors of observation.

A determination of the elements of the ellipse of the annual component of the polar motion was then presented, made from the newer observations, independently of the older ones previously used. The resulting elements are practically identical as to form, size and position of this ellipse. This seems to show that the axis of this elongated vibratory motion is stationary on the earth's surface, along a meridian forty-five degrees east of Greenwich. This negative evidence as to any apsidal motion seems to be of extreme importance in its bearing on the theory of the earth's rotation.

A demonstration was then presented of the fact that since 1890 the circular 428-day motion has been diminishing its radius in conformity to the requirements of the numerical theory derived from the observations between 1825 and 1890.

In addition to the above, a discussion was presented of 645 observations of the Pole Star made with the Pulkowa Vertical

*Abstract of a paper presented by Dr. S. C. Chandler.

circle between 1882 and 1891. This series is especially interesting and important in that it covers an interval during which we have very little other information, of an extensive character, as to the variations of latitude. A comparison of the curves of observation and theory thus provided for this decade exhibited a most striking accordance, and seems to leave no possible doubt that Nyrén's inference, that his observations do not betray evidence of the existence of the annual component of the polar motion, is erroneous and attributable to illogical methods in drawing his conclusions.

THE VARIATION OF LATITUDE AT NEW YORK, AND A DETERMINATION OF THE CONSTANT OF ABERRATION FROM OBSERVATIONS AT THE OBSERVATORY OF COLUMBIA UNIVERSITY.*

THE results given in this paper were obtained from 1,774 observations made between May 6, 1893, and June 20, 1894, with an 8-centimetre Wannschaff zenith telescope. The observations were planned for a determination of the constant of aberration by Küstner's method. Four groups of stars were used, having mean right ascensions approximately, as follows:

| | |
|---|---|
| Group I. | 6$^h$ |
| II. | 14 |
| III. | 18 |
| IV. | 22 |

Each group contained seven pairs of stars, and covered two hours in right ascension. The groups were observed both morning and evening, whenever the weather permitted. The original plan of observations required four observers, but it was unfortunately necessary to reject altogether the work of one observer. This caused considerable gaps in the series, in addition to those due to unfavorable weather conditions.

Only observations obtained during the period when it was possible to observe both evening and morning groups were used in calculating the latitude results employed for the computation of the constant of aberration. The observations of each group were gathered together into periods of about ten days each, in such a manner that the weighted mean of the dates should be the same for both the evening and morning groups. In this way the mean latitudes from the two groups should differ only on account of the difference between the declination systems of the two groups, and on account of any error in the assumed value of the aberration constant. They cannot differ on account of variation of latitude, provided any such variation is uniform during the short time of ten days. The result for the aberration constant is therefore independent of any assumption as to the law of latitude variations.

The constant of aberration was found to be $20''.457 \pm 0''.013$. The mean latitude of the observing station* was 40° 48′ 27″.195. The table of definitive latitudes for every ten days showed the variation of latitude, which was very small. The probable error of a single latitude was $\pm 0''.16$.

Observations have been continued from the date last given to the present time by Professor Rees and Dr. Davis, and will be kept up for some time longer. Reductions of the later series will be finished soon. A series of observations on the same stars has been made and is now being continued at the Royal Observatory at Capodimonte, Naples, by Professor Em. Fergola and two assistants, Messrs. Contarino and Angelitti.

NOTES OF EXPERIMENTS UPON THE RÖNTGEN RAYS.†

IN most investigations hitherto made for testing the question of the refraction of

* Abstract of a paper presented by Professor John K. Rees, Professor Harold Jacoby and Dr. Herman S. Davis.

* The observing station was at the new site of the University, about four miles north of the present University Observatory.

† Abstract of a paper presented by Professor A. W. Wright.

the rays the refracting body has been in the form of a prism. This involves the inconvenience that the absorption of the rays is so much greater toward the thicker portion of the prism as to cause imperfect or unequal definition in the image of the slit or wire used for the test. This has sometimes given rise to the appearance of a deflection of the rays away from the base of the prism instead of towards it, a negative action, implying a refractive index less than unity.

In the present experiments this was avoided by employing for the refracting bodies thick pieces of glass and Iceland spar with parallel sides, which were inclined at an angle of 45° to the path of the rays. The distances traversed by the rays in the two media were about 10 and 14 mm. respectively. The displacement of the image for ordinary light is about 1.5 mm. for the glass, and for the Iceland spar about 1.0 mm. and 1.8 mm. respectively, for the two images due to double refraction. A small platinum wire, stretched so as to be quite straight, rested upon the upper surfaces of the plates, and the rays from the tube were passed through a narrow slit in a copper plate. The slit was parallel with the wire and at a considerable distance from it. A strong and very clearly defined image of the wire was formed upon the photographic plate, showing no displacement by the glass plate or the Iceland spar, and no trace of widening or duplication by the latter, or, in other words, no perceptible effect of refraction, or double refraction.

Other experiments were described, in which very sharply defined images of fine platinum wires produced by the rays upon a photographic plate showed a faint central band, dark in the negative but light in the positive, corresponding to the familiar bright central band behind an opaque wire in the case of luminous rays. The converse effect of a dark central band in the positive from a narrow slit was also observed, but less distinctly. These results offer some support to the idea of true diffraction and the periodic character of the rays, but the matter must be regarded as somewhat uncertain until secondary maxima and mimima are obtained, which would settle the question of diffraction and permit the definite determination of wavelengths.

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*AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.*

A MEETING of the Council of the American Association for the Advancement of Science was held at Washington, D. C., on April 12th. Owing to the lamented death of Professor Edward D. Cope, the late President of the Association, Professor Theodore Gill presided as Senior Vice-President. A number of members were elected, and several matters of importance relating to the Detroit meeting were discussed and arranged at this meeting.

Professor Leland O. Howard, of the Department of Agriculture, Washington, D. C., was nominated as Vice-President and Chairman of Section F, to fill the vacancy caused by the death of Professor G. Brown Goode. Professor Howard was requested to prepare an address to be delivered before the Section at the Detroit meeting.

Professor I. C. White, Vice-President and Chairman of Section E, will go to St. Petersburg this summer as one of the delegates to the International Congress of Geologists. He will, however, prepare his Vice-Presidential address to be read before the Section at the Detroit meeting.

On Monday evening, August 9th, at the Detroit meeting, Professor Theodore Gill will deliver a memorial address on the life and work of Professor Cope, at the time and place appointed for the Presidential address, which was to have been given by Professor Cope. In this address Professor Gill will

give an account of the development of vertebrate paleontology, with which Professor Cope was so intimately connected.

Dr. Seth C. Chandler, of Cambridge, was elected Auditor of the Association, to fill the vacancy caused by the death of Dr. B. A. Gould.

The Permanent Secretary read the letters received from the British Association for the Advancement of Science in which it was stated that the General Committee of the British Association had voted to make the officers of the Detroit meeting of the A. A. A. S. honorary members of the B. A. A. S. for the Toronto Meeting, and to receive all fellows and members of the American Association as members of the British Associatian for the Toronto meeting by the payment of the regular annual assessment. It was voted that the Permanent Secretary should acknowledge the courtesy of the British Association and at the same time should express the hope that members of the British Association would be present at the Detroit meeting, calling attention to the provision in the constitution of the American Association relating to members of foreign associations.

Considerable discussion followed as to the proper method of extending courtesies to such members of the British Association as might take part in the Detroit meeting. It was voted that members of the British Association at Detroit should be invited to register as members of the several sections of the American Association, and that special attention should be paid by the officers of the respective sections to all foreigners thus registering.

The Permanent Secretary was instructed to arrange for the delivery of the Vice-Presidential addresses at the Detroit meeting in the afternoon as at former meetings.

F. W. PUTNAM,
*Permanent Secretary, A. A. A. S.*

## *CONGRESS OF AMERICAN PHYSICIANS AND SURGEONS.*

THE fourth triennial session of the Congress was held at Washington on May 4th, 5th and 6th. The meeting was well attended, as it deserved to be, the proceedings of the Congress as a whole and of the separate societies being interesting and profitable. Washington, especially in the spring, is the best possible place for such meetings, and this was recognized by deciding to hold the triennial meetings hereafter in that city. The only drawback is the lack of an adequate auditorium, which may be provided in the interval.

The proceedings of the Congress, as a whole, included several joint discussions. On Wednesday afternoon the Association of American Physicians, the American Physiological Society and the American Pediatric Society, united in discussing 'Internal Secretions considered in their Physiological, Pathological and Clinical Aspects.' The other subjects taken into consideration in the general sessions were 'The Gouty and Rheumatic Diatheses and their Relation to Diseases of the Eye,' Otology in its Relations to General Medicine,' 'Deformities of the Hip-Joint, especially Congenital Dislocations,' and 'The Classification of Acute General Peritonitis: The Prognosis and Treatment of the Different Varieties.'

The address by the President, Professor William H. Welch, on Thursday evening, was entitled, 'Compensatory and Protective Pathological Processes,' and was an admirable presentation of the subject, equally interesting to the practitioner and to the man of science concerned with the study of adaptations and evolution. The attendance at the meeting was large, although it and the subsequent reception were unfortunately simultaneous with a lecture by Sir Archibald Geikie and a reception to him in the rooms of the Geological Survey. Other ar-

rangements for the entertainment and social intercourse of members were a banquet on Tuesday evening at the Arlington and a 'Smoker' given by the Cosmos Club on Thursday evening.

On Wednesday afternoon a statue of the late eminent surgeon, Samuel D. Gross, was unveiled on the Smithsonian grounds near the Army Medical Museum. The statue was presented to the United States Government in a speech by Dr. C. N. Mastin, and was received by Surgeon-General George M. Sternberg. An address was made by Professor W. W. Keen, the successor of Gross at the Jefferson Medical College. It is to be hoped that many statues of men who have contributed to the advancement of science may ultimately find their place in the grounds of the Smithsonian Institution.

Reports of the proceedings of the American Physiological Society and of the Association of American Anatomists will be published in this journal. The papers presented before the various medical societies were so numerous that it is not possible to present a report of them, even though many of the contributions were not merely of interest to the medical specialist, but were also valuable contributions to science. The societies taking part in the Congress, their presidents and the numbers of papers offered before each, were as follows:

*The American Otological Society,*
Dr. Arthur Mathewson, Brooklyn, N. Y. 6 papers.
*The American Neurological Association,*
Dr. M. Allen Starr, New York City. 27 papers.
*The American Gynecological Society,*
Dr. James R. Chadwick, Boston, Mass. 21 papers.
*The American Dermatological Association,*
Dr. James C. White, Boston, Mass. 26 papers.
*The American Laryngological Association,*
Dr. Charles H. Knight, New York City. 18 papers.
*The American Surgical Association,*
Dr. John Collins Warren, Boston, Mass. 21 papers, including six subjects, followed by special discussion.
*The American Climatological Association,*
Dr. E. Fletcher Ingalls, Chicago, Ills. 26 papers.
*The Association of American Physicians,*
Dr. J M. DaCosta, Philadelphia, Pa. 36 papers.
*The American Association of Genito-Urinary Surgeons,*
Dr. Francis S. Watson, Boston, Mass. 20 papers.
*The American Orthopedic Association,*
Dr. Samuel Ketch, New York City. 45 papers.
*The American Physiological Society,*
Dr. Russell H. Chittenden, New Haven, Conn. 22 papers.
*The Association of American Anatomists,*
Dr. Frank Baker, Washington, D. C. 10 papers.
*The American Pediatric Society,*
Dr. Samuel S. Adams, Washington, D. C. 33 papers.
*The American Ophthalmological Society,*
Dr. George C. Harlan, Philadelphia, Pa. 38 papers.

## *ZOOLOGICAL NOTES.*

### COLOR CHANGE IN THE PLUMAGE OF BIRDS UNACCOMPANIED BY MOULT.

Not long ago Science noticed two papers, one by Dr. J. A. Allen and one by Mr. F. M. Chapman, in which the possibility of any change of color taking place in a feather after it was fully developed was emphatically denied. Now Dr. Arthur E. Chadbourne comes forward in the *Auk* for April, 1897, with facts which seem to make it evident that this alleged impossibility does take place. The author kept a pet Bobolink from January until the breeding plumage was complete, and writes as follows: "The bird always seemed well and strong, and the *color change was* NOT *accompanied by any increase in feather loss, i. e.*, not greater than during the winter, and often for several days in succession there were no cast-off feathers at all to be found. The total during the three weeks that the change was in progress was *thirteen*—namely, two broken rectrices and eleven contour feathers. It is hardly possible that any stray specimens were unnoticed, for even had they fallen outside of the cage they would have been found in the room, and a wire netting protected the window. 'Pin-feathers' could hardly have been overlooked, if present; for I often held the bird in my hand and

carefully examined it, blowing back the plumage until the skin could be seen. It is also safe to say, doubtless, that the cast-off feathers were not eaten by the bird itself. Hence it follows that unless the previous plumage was made up of only two tail and eleven body feathers, both of the former on the same side—which was certainly not the case—*my Bobolink was unquestionably an instance of color-change in the plumage without moult.*"

Dr. Chadbourne had already presented evidence tending towards the same end in *the Auk* for October, 1896, and January, 1897, wherein he discusses change of color in the Screech Owl, *Megascops asio*.

In *The Ibis* for October, 1896, Mr. John G. Millais also discusses the problem of color-change without moult, describing and figuring feathers from the Eared Grebe, *Colymbus auritus*, and Sanderling, *Calidris arenaria*, showing the great probability of such change taking place. The word *probability* is used advisedly, for Mr. Millais figures feathers in different stages from different birds, and while this evidence may be very strong it can not in the nature of things be so conclusive as change of color in the plumage of a bird kept under observation day after day.

In spite of all that has been written, the moulting and change of color in birds is comparatively little known, and it remains a fine field of research for the investigator who is willing to spend his time in the patient and careful collection of facts.

F. A. L.

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*NOTES ON INORGANIC CHEMISTRY.*

FOR some time past there has been a tendency on the part of an increasing number of chemists to attack the problems of inorganic chemistry, profiting by the light which the study of organic chemistry has thrown upon the carbon and nitrogen atoms. This is an encouraging tendency from the standpoint of theoretical chemistry, for while the devotion of by far the largest proportion of chemists, for several decades down to the present time, to organic chemistry has widened vastly our knowledge of organic compounds and the carbon atom, yet the study of all other atoms is even more necessary for the theory of chemistry. Relatively very few inorganic compounds have been studied and some of our most familiar reactions are illy understood. So far from the inorganic field having been long ago worked out and exhausted, it is here that the chemistry of the future will find its most prolific harvest. Yet the field is far harder to till and less productive of immediate results.

THE *Berichte* of the German Chemical Society might almost seem to be devoted to organic chemistry, so large is the preponderance, yet we find that the inorganic field is not wholly neglected. In the last number Muthmann and Seitter contribute an investigation of the sulfid of nitrogen, which is in part a development of earlier work of Demarçay. When nitrogen sulfid $N_4S_4$ is treated with chlorin a theachlorid $N_4S_4Cl_4$ is formed, as shown by Andreocci. When sulfur chlorid is used, a compound of the formula $N_3S_4Cl$ is obtained, and from this a series of derivatives, including the bromid, iodid, nitrate and thiocyanate. There thus appears to be a comparatively stable univalent group, $N_3S_4$, which the authors believe to have a ring formula analogous to that of benzine.

IN the same *Berichte*, Pawlewski, of Lemberg, gives a careful study of the physical properties of sulfuryl chloride, $SO_2Cl_2$, and some of its chemical reactions. Professor Söderbaum, of Gothenburg, in the same number describes a reaction between acetylene and cupric salts. The cuprous acetylid has been long known, but that acetylene gives a precipitate with cupric salts has

apparently been overlooked. The compound formed seems to have a very complex formula, being represented by $C_{17}Cu_8H_4O_3$. It is more explosive than the corresponding cuprous compound and, unlike it, on treatment with dilute acid yields very little acetylene. It gives, on the contrary, a humus-like substance of a formula of about $C_{12}H_4O_3$, which resembles both humic acid and the so-called graphite hydrate obtained from the graphite of cast iron. It would seem to be an unique case of the condensation of acetylene, at ordinary temperature under the influence of a copper salt, to a compound of high molecular complexity.

J. L. H.

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*ASTROPHYSICAL NOTES.*

In No. 367 of the Proceedings of the Royal Society is a note by Professor Oliver Lodge, read on February 11th, in which he calls attention to the notable discovery by Professor P. Zeeman, of Amsterdam, that lines in the spectrum of a flame may be broadened when a magnetic field is concentrated upon the flame.

Zeeman's paper appears in the *Philosophical Magazine* for March (Vol. 43, pp. 226–239). He alludes to the fact that similar experiments were the last researches of Faraday, in 1862. With the relatively slight dispersion then available, however, the effects could not have been observed.

Sodium and lithium were used by Zeeman, and the broadening effects were observed in both the emission and absorption spectra, which were obtained from a powerful concave grating.

The experiment was also tried on the band spectrum of absorbing iodine vapor, with negative results, which, however, confirmed the accuracy of the experiments with sodium. The widening of the sodium lines to both sides amounted to about $\frac{1}{10}$ of the distance between $D_1$ and $D_2$ (that is, to about 0.15 tenth-meters). As the intensity of the magnetic field was about $10^4$ c.g.s. units, there would be a positive and negative magnetic change of $\frac{1}{10000}$ of the period.

The theory of the motion of ions or electrons, whose vibrations are those of light, is discussed according to the views of Professor Lorentz, who pointed out to Zeeman that if the theory was true the edges of the widened lines ought to be circularly polarized in the direction along the lines of magnetic force, and plane polarized in directions normal to the lines of force. This was clearly shown by experiment to be the case, and it has been confirmed by Lodge, who also readily obtained the broadening effect in the sodium flame.

These researches are decidedly suggestive, and have an important astrophysical as well as physical application. The view is held by many that strong magnetic forces occur in the sun (and hence by analogy in the stars). Thus a new cause may perhaps be assigned for the wide range and variations in the breadth and intensity of spectral lines of celestial bodies.

E. B. F.

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*SCIENTIFIC NOTES AND NEWS.*

LEGISLATION ON THE FOREST RESERVATIONS.

The Senate, on May 6th, adopted Senator Pettegrew's amendment to the Sunday Civil Appropriation Bill, suspending President Cleveland's order of February 22d, setting aside some 20,000,000 acres of timber lands in the Northwest as forest reservations. The N. Y. *Evening Post* calls this action 'monstrous,' and it seems to be generally misunderstood. The Senators from the States concerned favor forest reservations, but President Cleveland's order, with the laudable purpose of adequately celebrating Washington's Birthday and securing to his administration the credit of this important movement, seems to have been premature. The letter from the Secretary of the Interior to the President of the National Academy of Sciences requested an official expression of the Academy upon the following points:

1. Is it desirable and practicable to preserve from fire and to maintain permanently as forested lands those portions of the public domain now bearing wood growth for the supply of timber?

2. How far does the influence of forest upon climate, soil and water conditions make desirable a policy of forest conservation in regions where the public domain is principally situated?

3. What specific legislation should be enacted to remedy the evils now confessedly existing?

It came within the province of the committee to recommend the setting aside of additional forestry reservations, but its primary object was to suggest an intelligent policy for the preservation of the reserves previously made. The Academy was especially asked for an opinion on the legislation recommended by the American Forestry Association and by the American Association for the Advancement of Science, in the hope that the weight of the Academy's influence would induce Congress to enact the needed legislation. Recommendations to this effect will doubtless be in the report now prepared by the committee of the Academy, and will guide the President and the Secretary of the Interior in the administration of the reserves in accordance with the powers given them by Senator Pettigrew's amendment.

THE BEQUESTS OF THE LATE PROFESSOR COPE.

THE will of Edward D. Cope, signed October 1, 1895, in accord with the guiding principles of his life, leaves, after making ample provision for his family, his collections for the benefit of science. His scientific books, his osteological collection, and his collection of fresh-water mollusca are given to the School of Biology of the University of Pennsylvania, and his collection of minerals to the University. Duplicates of the collection of fresh-water mollusca are to go to the Cincinnati Society of Natural History and to the American Museum of Natural History. The collections preserved as wet preparations and the skins of animals are given to the Philadelphia Academy of Natural Sciences. The paleological collections are divided into three parts: *First.* The North American collection. *Second.* The South American, *i. e.*, the Pampean collection which was purchased of the Buenos Ayrean exhibitors at the Paris Exposition of 1878, and small collections from the West Indies and Mexico. *Third.* European collections chiefly from the Neocene of Alber of France. It is directed that these collections be sold, and after the payment of private bequests, including $2,500 to each of his assistants, Mr. Jacob Geismar and Miss Anna M. Brown, that the balance, estimated at $40,000, be given to the Academy of Natural Sciences at Philadelphia, as an endowment for a professorship or curatorship of vertebrate paleontology. The incumbent must be an original investigator elected by the Council of the Academy and approved by the National Academy of Sciences, $400 of the income of this endowment to be used for the procurement, either by collection or purchase, of vertebrate fossils. The executors of the will are Mr. John B. Garrett and Professor Henry F. Osborn.

GENERAL.

A MEMORIAL meeting in honor of the late Professor Sylvester was held under the auspices of the Johns Hopkins University on May 1st. Addresses were made by Dr. Fabian Franklin and others.

THE Flower Astronomical Observatory of the University of Pennsylvania was dedicated on the afternoon of May 12th. An address was made by Professor Simon Newcomb.

PRINCE LUIGI with a large party will shortly arrive in this country, with a view to making explorations in Alaska and ascending to the summit of Mt. St. Elias. Professor I. C. Russell has made thorough explorations of the mountain, attaining a height of 12,000 feet, but the summit, some 18,000 feet in height, has never been reached.

THE Literary and Philosophical Society of Sheffield proposes to have painted for the Society a picture of Dr. H. C. Sorley in celebration of the fiftieth anniversary of the beginning of his scientific work.

THE degree of LL.D. has been conferred by the University of Edinburgh on Professor James Dewar and Dr. John Willie.

THE Royal Geographical Society has elected the following as honorary corresponding members: Professor G. Della Vedova, Baron Toll and Captain Otto Irminger.

Dr. Nansen's proposed lecture before the Geographical Society at Rome has been abandoned because the Society was unable to pay the terms demanded.

The Board of Estimate and Apportionment of New York City have authorized the issue of bonds to the amount of $500,000, the money to be used for the erection of a further wing for the American Museum of Natural History. Plans for the wing prepared by the architects, Messrs. Kodeberg and See, were submitted and approved.

The class of 1897 of Yale University has presented to the Peabody Museum a meteorite, weighing 65 lbs., which was found three years since in Kansas.

A proposition in the Massachusetts House of Representatives to amend the bill appropriating $150,000 for the work of exterminating the gypsy moth, by making the amount $200.000, was voted down, but the bill itself was ordered to a third reading.

It is reported that the authorities of the elevated railways of New York City have examined Mr. Keely's motor with a view to its introduction. It is probable that the nature of motor was not made clear to them and that it will not be used in New York. Scientifically inclined people are not likely to believe that Mr. Keely's motor can make something out of nothing, but this will not be the opinion of those who have invested money in the scheme.

At the monthly general meeting of the Zoological Society of London, held on April 22d, it was reported that the additions to the Society's menagerie during the month of March were 152 in number. Special notice was directed to three examples of the Indian pygmy goose (*Nettopus Coromandelianus*), presented by Mr. Frank Finn. It was stated that many attempts had previously been made to introduce this bird into Europe, but without success, and that these were the first specimens which had reached the Society's gardens alive.

Another great Auk's egg has been sold at auction in London—this time to Mr. G. T. Middlebrook for 280 guineas.

The appearance of two new serials is noted in *Natural Science: The Aeronautical Journal*, published by the resuscitated Aeronautical Society, and *East Asia*, a quarterly, which will include the natural history, etc., of the region.

Dr. C. Du Bois-Reymond, son of the late Emil Du Bois-Reymond, expects to edit, from the notes of students, the courses of lectures given by Du Bois-Reymond, at the University of Berlin, on the Results of Modern Science and Physical Anthropology.

It gives us regret to announce the death of Martin L. Linell, Aid in the Department of Insets of the U. S. National Museum for the past nine years. Mr. Linell was 47 years of age, a Swede by birth and a former student in the University at Lund. He came to America in 1879, and was at first Curator of the Brooklyn Entomological Society, accepting the position in the National Museum in 1888. He was one of the most learned coleopterists in the country, and his high scientific attainments, as well as his thorough acquaintance with the great collection of insects at Washington, will render it very difficult to fill his place.

The *American Naturalist* for May contains a short appreciation of the late Professor Cope, by Dr. Persifor Frazer, who takes *pro tem.* Cope's place as senior managing editor of the journal, and an article on Cope by Professor J. S. Kingsley, who for many years was associated with Cope in the editorship of the journal. The number also contains six portraits of Cope. Four of these are from photographs taken respectively in 1879, 1884, 1887 and 1892. These show an evident gain in force of expression, which appears not to be unusual with men of great achievement. The frontispiece of the number is from an oil painting by Mr. George W. Pettit, in the possession of the American Philosophical Society, and a second full-page plate is from a bust by Mr. Eugène Costello.

The opening article of the May number of the *American Journal of Science* is an admirable memoir of Hubert Anson Newton, by Professor J. Willard Gibbs. The frontispiece of the number is a portrait of Professor Newton in his library, taken by a member of his family in the spring of 1894. The appended bibliography contains 69 titles.

FROM a note in the current number of the *American Journal of Science* we take the following facts regarding the late Matthew Carey Lea, who died on the 15th of March. He was born in 1823, and was the eldest son of Isaac Lea, the publisher, well known as a geologist and a mineralogist, but especially as a conchologist in connection with his investigations on the genus *Unio*. His early associations giving him a special interest in scientific matters, he entered the laboratory of Professor James C. Booth and there acquired great proficiency in chemistry. To this science he devoted his life, his chemical researches being numerous and important. He was elected to membership in the National Academy of Sciences in 1892, and the list of his more important papers then published contained fifty-four titles. These investigations for the most part related to the chemistry of photography, and especially to the action of light and other forms of energy upon silver salts. He described photo-bromide and photo-iodide of silver, and in 1887 published a paper on the 'Identity of the photo-salts of silver with the material of the latent photographic image.' His most remarkable discovery, made in 1889, was that silver is capable of existing in three allotropic states.

ON the catalogues of Muscineæ by Mitten, in Godman's Natural History of the Azores, which recognized 47 species from the archipelago, and the recent collections of Trelease, Brown, Carreiro, Machado, Blanchy, Richard and Minelle, M. Cardot in the last report of the Missouri Botanical Garden bases a catalogue of 88 Azorean species and 14 varieties or forms, of which 9 species and 3 varieties are considered as new to science. He also describes one new species and indicates one other as probably undescribed, from a small collection of 19 species made on Madeira last year by Trelease. Eleven plates illustrate, in habit and detail, the novelties.

A LETTER from Professor Th. Tschernychew to Dr. Persifor Frazer in reference to the approaching International Geological Congress is translated and published in the current number of the *American Naturalist*. It states "that from this time on the number of persons who have inscribed themselves is so great—nearly 700—that it will be absolutely impossible to enable them all to take part in the excursions." It is not clear from this whether those already registered may be excluded from the excursion or whether this applies only to new applicants, but those who are attracted to the Congress by these excursions will do well to make enquiry.

THE issue of SCIENCE for February 12th contained an article by Professor A. B. Macallum on the arrangements for the Toronto meeting of the British Association. We are now able to give the preliminary daily programme, which is as follows:

*Wednesday, August 18th.* Reception Room open, 8 a. m. to 6 p. m.; Meeting of Council at 10 a. m.; Meeting of General Committee at 3 p. m; Address of the President, Sir John Evans, in Massey Hall, at 8 p. m.

*Thursday, August 19th.* Sectional Meetings in most cases, 10 a. m. to 3 p. m.; Garden Party, 3.30 to 6 p. m; Conversazione in the Legislative Buildings, 8.30 to 11.30 p. m.

*Friday, August 20th.* Sectional Meetings in most cases, 11 a. m. to 3 p. m.; Garden Party, 3.30 to 6 p. m.; Lecture by Professor W. C. Roberts-Austen, C.B., F.R.S., in Massey Hall at 8.30 p. m.

*Saturday August 21st.* Sectional Meetings, 10 a. m. to 1 p. m.; Excursion of Members of Section G. (Mechanical Science) to Niagara, 9 a. m. to 6 p. m.; Excursions to Hamilton and neighborhood, Niagara Falls and Muskoka Lake Region, returning on Monday morning. Lecture to Workingmen. Lecture and subject to be announced.

*Monday, August 23d.* Sectional Meetings in most cases, 10 a. m. to 4 p. m; Excursion of Members of Section C (Geology) to Scarboro' Heights, 1 to 6 p. m.; Garden Parties; Lecture by Professor J. Milne, F.R.S., on Earthquakes, in Massey Hall, at 8:30 p. m.

*Tuesday, August 24th.* Sectional Meetings in most cases, 10 a. m. to 3 p. m.; Garden Party at Trinity College, 4 to 6 p. m.; Conversazione in the University Building, 8:30 to 11:30 p. m.

*Wednesday, August 25th.* Some Sectional Meetings, 10 a. m. to 1 p. m.; Concluding General Meeting, 2:30 p. m.; Garden Parties, 3:30 to 6 p. m.; Banquet in honor of Lord Kelvin, Lord Lister and Sir John Evans, 8 p. m.

*Thursday, August 26th.* Excursions to Niagara Peninsula, Thousand Islands, Ottawa, Montreal, Upper Lakes and to Manitoba and British Columbia. (See Excursion Guide.)

## *UNIVERSITY AND EDUCATIONAL NEWS.*

Mrs. E. B. Coxe has given Lehigh University $20,000 as a fund in memory of her husband, Eckley B. Coxe, the income of which is to be used for the support of poor and deserving students.

Colorado College has been given $10,000 by an anonymous donor to be used for a building for women students.

The Massachusetts Institute of Technology receives, by the will of the late William Tappen, Jr., Milton, Mass., $10,000 to be used for deserving students, and is further made the residuary legatee of the estate.

The will of the late Rev. Caleb Bradley, of Dedham, Mass., gives $2,000 to Tufts College and $2,000 to Gales College.

Dr. E. Fischer has been promoted to a full professorship of botany in the University at Berne, and has been made Director of the Botanical Gardens. Dr. Gustav Jäger, docent in the University of Vienna, has been appointed to an assistant professorship of theoretical physics, and Dr. Friedrich Gräfe, docent in the Polytechnic Institute at Darmstadt, to an assistant professorship of mathematics.

Mr. A. Francis Dixon has been appointed professor of anatomy in the University College of South Wales. It appears that the method of election was for three selected candidates to appear before the Council, one to be chosen by that body.

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## *DISCUSSION AND CORRESPONDENCE.*

### ON SUPPOSED EFFECTS OF STRAIN IN TELESCOPIC OBJECTIVES.

To the Editor of Science: In your issue of April 23d (page 656) I notice a criticism by Professor E. S. Holden, Director of the Lick Observatory, which seems to me to call for a word of comment. Professor Holden is inclined to discredit the observations of Mercury and Venus made by Mr. Percival Lowell and his assistants at Flagstaff, principally for the reason that they have not as yet been 'fully confirmed by other observers with other telescopes.' The markings seen by Mr. Lowell he attributes to a supposed strain on the glass, induced by an overtight condition of the adjusting screws or of the objective in the cell.

Now it happens that I personally superintended the adjustment of the Lowell objective in the cell at Flagstaff before the observations in question were made, and I am satisfied that the screws holding the glass in place were *barely turned home with the fingers*. I desire to express it as my belief, founded on long experience as a practical optician, that strain in the glass is incapable of producing the effect of markings on a planetary disc. It is obvious that the same class of strain which exists in the Lowell must be present also in the Lick objective, since both are mounted precisely alike in their cells on triangular bearings; and if such effects were produced in the 24-inch glass as Professor Holden imagines they would be much more apparent in the 36-inch.

Having worked both of these objectives myself, and expended as much artistic ability on the one as on the other, there can be no impropriety in my saying that the performance of the Lowell glass is equal to that of the Lick or any of our large telescopes.

Alvan G. Clark.

Cambridgeport, Mass.,
May 1, 1897.

### THE LOESS FORMATION OF THE MISSISSIPPI REGION.

To the Editor of Science: In reply to Professor J. E. Todd's letter in your issue of April 30th I wish to offer the following remarks:

A complete and satisfactory answer of the questions presented by Professor Todd would require a thorough discussion of the Loess formation; but the necessarily limited nature of this communication, and my own imperfect knowledge of the formation in its entirety, will admit only of my touching briefly upon a few points.

There is, in portions of the upper Mississippi region, particularly in that part of it with which I am best acquainted — northwestern Illinois, a silt deposit which is spread out over the very uneven uplands as an originally nearly uniform sheet, and whose relation to belts of comparatively thick typical loess along the

main streams is similar to that between the gray loamy clay and the 'loess' of Missouri. The upland deposit is similar in composition to the typical loess of the valleys, except that it contains a large constituent of fine clay particles which partially bind the rounded silt grains. This deposit of unusually argillaceous loess, besides mantling the upland ridges, descends the slopes of the valleys and is present over the thick terrace-like deposits of true loess. They are separated by a smoothly undulating but quite sharp line. This line is present throughout northwestern Illinois and in Iowa at Dubuque. Without discussing the significance of this line, I will call attention to the fact that Chamberlin and Salisbury, in making the study of the loess of the 'Driftless Area,' recognized the distinction between the silt deposits along the main streams and on the uplands remote from the main water-courses, particularly on the east side of the Mississippi river; and in reference to their origin they say, on pages 306–7 of the 6th Annual Report of the U. S. Geological Survey, "the loess-depositing waters were neither true lakes nor true rivers, but assumed an intermediate fluvio-lacustrine character, possessing sufficient onward flow to prevent the deposition of a large proportion of the clayey constituents of the silt they bore. * * * The loess probably represents the debatable ground between the two" (a lake and a river). "The coarser stratified portion along the immediate valleys of the great streams seems clearly on the fluvial side of the line, while the broad, loam-like mantle apparently lies on the other side." It is this comparatively thin but uniform mantle of argillaceous loess or loam, on the uplands and in the valleys, for whose origin I claim a purely lacustrine or possible semi-marine character, largely because, while clearly water-laid, it passes within one-half mile and less from the top to the base of hills 100 and even 200 or more feet in height. It mantles a region having a range in altitude of 600 or more feet.

With special reference to the question of the mode of formation of the gray loamy clay of Missouri, which I regard as a portion of the Loess formation, as also does Professor Todd, I wish to make the following statements, which, I believe, can easily be demonstrated to be facts:

The drift sheet of northern Missouri was not laid down on a perfectly level plain, but on an undulating upland, dissected by valleys of which the Missouri and Mississippi were examples. The Missouri valley, at least from Boonville onward, is preglacial inage, and it is hardly necessary for me to add that the Mississippi valley along the entire east border of the State is so also. These valleys do not now have much drift because it has been removed from them by subsequent erosion.

The line of Professor Todd's supposed 'barrier' or ridge from the Osage-Gasconade divide to Pike county, Illinois, being traversed by two important preglacial valleys—the Mississippi and Missouri—loses its effectiveness to account for the difference in altitude of the loess plains of northern Missouri and southern Illinois.

The gray loamy clay or 'upland loess' of northern Missouri was laid down on an undulating plain, some portions of which have a decided slope. The 'fluvial' theory of its origin, which supposes that the area upon which it was being formed was dry during a large part of the year, and, therefore, that it was deposited somewhat like the alluvial silts of the Mississippi delta at the highest flood stages, is opposed by the inequalities, when considered over broad areas, of the surface of the sheet of loess or loam. Rivers do not wander about except on plains which have an even and very slight slope. In order to uniformly sheet with water-laid silt even a very slightly hilly region, a lake-like body of water is required. That which covered northern Missouri at the time the 'upland loess' was deposited may not have exceeded 50 or 100 feet in average depth, and may not have covered the entire loess region at one time, but its nature was decidedly different than that of a river.

The failure to find beach ridges is far from being positive evidence of the absence at any former time of the shore line of a great lake, or even of the sea. For instance, the ocean waters in advancing over Florida to form the Columbia sands which mantle the northern portion of the peninsula, and later in withdrawing

from the same area, formed no distinct beach ridges; at least none have been observed by the writer. In Citrus and neighboring counties the marine Columbia sands seem to be composed of three members—a lower or red member (formed while the shore line was advancing inland), a middle or yellow member (formed mainly during the culminating period of the submergence), and an upper or white member (formed while the shore line was retreating). These rest upon an undulating land surface, rising in a distance of about eight miles, from sea level at Crystal River, to an altitude of about 180 feet A. T., near Lecanto, and again descending to nearly sea level in the Withlacoochee valley. No prominent beach ridges were formed *because the shore line did not remain at one level on the slope of the land a sufficient length of time.* This is only one of a number of similar cases which might be mentioned where the sea advanced upon and retreated from a sloping land surface without forming beach ridges.

In regard to the other objections against the lacustrine origin of the Missouri 'upland loess,' presented by Professor Todd, namely, that it is absent from certain areas in the eastern part of the State near the junction of the Mississippi and Missouri rivers and at a low level, I wish to suggest that it may have been there present originally as a very thin sheet which has since almost disappeared because of erosion. Past experience has taught me that when the loamy deposit which constitutes the 'upland loess' is very thin and patchy and approaches closely in composition and appearance to the residuary clays it is, by many geologists, scarcely recognized as a portion of the Loess formation. This suggestion is justified by the fact that I have observed in the region traversed by the St. L. and S. F. railroad, between Cuba and Pacific City, a loamy clay, usually free from pebbles, apparently separated from the residuary clay by a sharp line, and overspreading the surface of a hilly country like the 'upland loess' in northwestern Illinois. If this is a portion of the Loess formation it indicates that the lake or sea waters rose against the northeastern corner of the Ozark uplift to an altitude of 1,000 or more feet, or rather I should say that the land went down to that amount below these waters. The superficial silts of this region ought to be investigated. O. H. Hershey.

Freeport, Ill.

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## *Scientific Literature.*

*Birds of the Galapagos Archipelago.* By Robert Ridgway. Proc. U. S. National Museum, Vol. XIX., pp. 459–670, 2 pls. Cover title-page dated 1896, but not published till March, 1897.

It was in the Galapagos Islands that Darwin, during the memorable 'Voyage of the Beagle' in 1835, made the original observations which led him to discover and formulate the great principle of evolution—to the elucidation of which the remainder of his life was so successfully devoted—and it was the birds of these islands which first drew his attention to the derivative origin of species. Hence to naturalists, and particularly ornithologists, the Galapagos Islands will ever be classic ground.

Unhappily, the advent of man, with his retinue of goats, pigs, cats and rats, has already resulted in the extermination of at least one of the indigenous birds; others are threatened with the same fate. The extinction of an animal is always a matter of regret, and in the present case is a serious loss to science because of certain unsolved problems respecting the extraordinary and unique interrelations of many of the species. For instance, certain genera contain a large number of forms, alike in color and markings, but differing in proportions, particularly the size and shape of the bill. In one genus (*Geospiza*) the bill presents a series of types which at first would hardly be supposed to fall within the limits of a single family, —much less a single genus. This remarkable series comprises bills that in form and size resemble those of tanagers, small-billed sparrows and huge-billed grosbeaks. But for the existence of intermediate forms no one would think of placing such diverse kinds in a single genus. In one or two of the genera almost every conceivable stage and step of intergradation exists, so that it becomes extremely difficult—if not in some cases impossible—to draw the line between specific and individual variations.

Owing to the absence of information concerning the adaptations and limits of specific varia-

tion among these birds in life, and owing further to the fact that certain species are on the verge of extermination, while others doubtless still remain to be discovered, it is clearly of pressing importance that some competent ornithologist should visit the islands and study the habits and interrelations of the various forms before it is too late.

In recent years relatively large collections of birds have been made on some of the islands. Those obtained by the naturalists of the 'Albatross' in 1888 and 1891, and by Dr. George Baur and Mr. Adams in 1891, were brought to the U. S. National Museum, where they have been critically studied by Mr. Ridgway. The result is the present admirable paper entitled '*Birds of the Galapagos Archipelago*,' one of the most important ornithological publications that has appeared in many years. It is a comprehensive treatise 'intended,' as the author tells us, 'to embody practically all that it known of the avi-fauna' of the region. Still "it does not claim to be exhaustive, for a great deal has yet to be learned before anything like a complete exposition of the subject is possible."

Respecting the conflicting theories as to the origin of the Galapagos Mr. Ridgway states: "All writers are agreed that the Galapagos Islands are volcanic; nearly all, from Darwin to Agassiz, agree that they were uplifted from the sea by volcanic action, and that their upheaval therefore antedates the advent of organic life upon them. Dr. Baur, however, believes that these islands are the higher points of an extensive submerged area, whose subsidence took place after a fauna and flora had been acquired; or, to quote his own words: 'At a former period these islands were connected with each other, forming a single large island, which itself at a still earlier time was united to the continent, probably with Central America and the West Indies.'"

Mr. Ridgway modestly adds that he is "not competent to discuss the relative merits of these two opposite theories from the physiographer's standpoint; but if the apparent relationships of the fauna have any bearing on the question," he believes Dr. Baur's theory 'at least worthy of serious consideration.' He calls attention to the map accompanying Alexander Agassiz's report on the cruise of the 'Albatross' for 1891, which shows that the Galapagos and Cocos Islands stand on a submarine plateau covered by only 1,500 fathoms of water, and that this plateau reaches northeasterly to within 100 miles of the present coast of Central America, and is only a little more than 30 miles distant from the 1,500-fathom coast-line. These distances are too insignificant to form any barrier to the free passage of birds; hence, if it can be assumed that the present submarine plateau was ever above the level of the sea it would naturally have received its original bird population from Central America. Let us see how this accords with the facts of present distribution. Mr. Ridgway tells us that 38 genera of birds are believed to breed on the islands. Of these, 23 are wide ranging, 6* are peculiar to the islands, and 7 are tropical American. Eliminating the wide-ranging genera, 13 remain, of which 6 are peculiar and all of the others tropical American. Mr. Ridgway then takes up the 5* peculiar genera and discusses each with respect to its relationships and probable origin. After stating that two of these are clearly of American origin, that two resemble both American and Hawaiian types, and that the remaining one "has no very near relative among the known continental or West India birds, but in general appearance is very much like a smaller 'edition' of the Hawaiian genus *Oreomyza*," he sums up as follows: "Of the five peculiar Galapagoan genera of birds, only two (*Nesomimus* and *Nesopelia*) are of evident American relationship. The remaining three have so obvious a leaning toward certain Hawaiian dicæidine forms that the possibility of a former land connection, either continuous or by means of intermediate islands as 'stepping stones,' becomes a factor in the problem. It may be that the resemblance of *Cocornis*, *Cactornis* and *Camarhynchus* to the above mentioned *Hawaiian* forms is merely a superficial one, and not indicative of real relationship. I do not by any

* By a singular slip the number of peculiar genera given in the tables and on page 465 is 6, while two pages later it is reduced to 5. The latter number is the result of uniting *Cactornis* with *Geospiza* after the earlier pages were written. By an oversight this was not corrected in the proof.

means claim, on the strength of such evidence, a common origin for them, but merely present the facts as 'food for reflection.'"

In discussing the extraordinary conditions presented in the genus *Geospiza*, Mr. Ridgway states: "When it is remembered that the coloration is practically, if not absolutely, the same in all of the twenty-odd forms of the genus *Geospiza*, it will be seen that if any segregation of species is made at all it must be based upon measurements; and when it is further seen that there is a gradual transition from the enormous beak of *G. magnirostris* to the comparatively minute one of *G. parvula* and from the excessively thick one of *G. pachyrhyncha* (whose lateral outlines approximate an equilateral triangle) to the slender and curved one of *G. scandens* or the acuminate one of *G. acutirostris;* and that size of beak is not necessarily correlated with length of wing, tarsus, etc., the difficulty of defining species becomes obvious. * * * Having been perplexed by these difficulties I have carefully weighed all doubtful cases, and whenever there seemed to be a well defined average difference between specimens from different islands I have not hesitated to separate them as local forms. No other course, indeed, is practicable; for were 'lumping' once begun there could be no end to it, unless purely arbitrary limits were given to the species recognized, and if followed to a logical conclusion might easily end in the recognition of a single variable species, equivalent in its limits to the genus."

In an earlier paper Mr. Ridgway united the genera *Cactornis* and *Geospiza* because of intergradation; in the present paper *Cactornis* is retained in a subgeneric sense. Mr. Ridgway quotes a letter from Dr. Baur as follows: "You place the species of these two genera in one genus, *Geospiza*. I do not think that this is natural. Both have their peculiar representatives on the different islands, and if you place them together this peculiar differentiation of each is lost sight of. *Cactornis* is more slender than *Geospiza* and has many more black individuals. I would keep the two genera apart." Dr. Baur might have strengthened his position by adding that genera should be based on degree of differentiation rather than on the survival or non-survival of intergrades. The present case is unusually extreme, because a complete chain of intermediate 'species' is known to exist, connecting the slender-billed *Cactornis* type with the thick-billed *Geospiza* type. But in all cases of derivative genera the theory of evolution, conceived on these very islands, calls for the existence at one time or another of similar chains of intergrades. Whether such intergrades survive, bridging the extremes with living forms, or die off, leaving the extremes trenchantly defined, is a matter of zoological interest, but one in no way affecting the degree of differentiation of the extremes, on which alone, in my judgment, the question of generic distinctness should rest.

Chronologic lists of the species found on each island by the various explorers, from Darwin in 1835 to Townsend, Baur and Adams in 1891, are given, and also an exceedingly convenient tabular statement showing in 16 columns the various islands on which each of the 105 species is known to occur. This is followed by the systematic part of the paper, comprising keys, maps and careful descriptions of the various genera and species. The map used is a skeleton, covering considerably more than a half page, on which range is indicated by heavy-face numerals. Since this map is introduced about 50 times the waste space at the top and bottom adds materially to the size of the paper. Two plates are given showing the astonishing variations in the size and form of the bill in the genera *Nesomimus*, *Camarhynchus* and *Geospiza*. The paper closes with a bibliography of 23 titles. The technical descriptions are models of thorough painstaking work, and the memoir as a whole easily takes a place among the classics of ornithology.

By an utterly unpardonable blunder, for which the author is in no way responsible, the cover-title-page is dated 1896, although not published until March, 1897. Accidents are liable to occur anywhere, but the number of papers bearing the cherished imprint of the Smithsonian Institution which are permitted to appear under ostensible dates that materially antedate the actual date of publication is not only an injustice to the author and an annoy-

ance to the bibliographer, but is hardly creditable to the spirit of scientific accuracy and fairness which American science has done so much to promote.

C. H. M.

*The Mechanics of Pumping Machinery.* By J. WEISBACH and G. HERRMANN. Authorized Translation by K. P. DAHLSTROM, M. E. Macmillan & Co. 1897. Pp. 300. 8vo. $3.75.

This work is a translation of the latest division of the Weisbach cyclopedic treatise on Engineering Mechanics. It is intended mainly as a text-book, and for use in advanced courses of instruction in engineering schools; while it is also thought that it may have value to the designer and constructor in his daily work. The translator has added some matter exhibiting the progress made in this field since the original publication of the book in Germany, and in this he has had the aid of Professor Klein's notes. The work includes discussions of early forms of water elevators and hydraulic machinery, of the theory and action of pumps, both reciprocating and rotary, and an account of other less well-known apparatus for raising water. The reputation of the author, Professor Herrmann, the distinguished technicist, is a guarantee of the reliability of these discussions, and this guarantee is confirmed by examination of the pages of this translation, in which these discussions have been faithfully brought over into the English and in satisfactory form.

The illustrations are numerous and helpful; the text is by them rendered admirably lucid. In general appearance and style the volume corresponds to its predecesors in the same series and, without being elegant, is creditably made up. Its price is moderate and it will probably find its place in the library of all who possess its companion volumes.

R. H. T.

## SCIENTIFIC JOURNALS.

### THE JOURNAL OF COMPARATIVE NEUROLOGY. VOL. VII., NO. 1.

THE issue for April contains three memoirs, besides editorials and reviews. B. F. Kingsbury writes on 'The Structure ard Morphology of the Oblongata in Fishes,' from the standpoint of the components of the nerve roots. Some 17 species of cartilaginous and bony fishes were examined, their nerve roots analyzed and the components traced to their respective centers, along the lines laid down by Strong's recent work on the cranial nerves of Amphibia. It will be remembered that Strong reduces the sensory nerves of the head to three types: (1) the general cutaneous system, innervating the skin and terminating in the 'ascending' or spinal fifth tract of the medulla; (2) the acustico-lateral system, innervating the lateral line canals and the ear and terminating in the tuberculum acusticum of the medulla; (3) the fasciculus communis system, innervating taste buds, certain specialized end-organs of the skin not belonging to the lateral line system, and the mucous and visceral surfaces in general, and terminating in the fasciculus communis of the medulla, or the cellular aggregates associated with it (lobus vagi of fishes).

Now in the fishes examined, Dr. Kingsbury finds these components present, and arranged in essentially the same way as in the Amphibia. The varied and apparently anomalous conditions found in the medulla of the fishes, which have so long puzzled the morphologists, have been reduced for the most part to variations in the relative development of these three factors. The lobus trigemini of the catfishes is regarded as a specialized portion of the fasciculus communis system. These conclusions have been reached by a study of the central relations only of the nerve roots. It may be added that researches now in progress at Columbia University, upon the peripheral distribution of these roots in the bony fishes, have fully substantiated most of his discoveries.

Dr. Kingsbury follows with a second paper entitled 'The Encephalic Evaginations in Ganoids.' The new and important points are two: (1) The presence in the adult Amia of the first epiphysial vesicle of Hill and its innervation from the left Habena; and (2) the existence in Amia and Lepidosteus of lateral cephalic and caudal extensions of cavity caudad of the velum transversum of Kupffer, constituting considerable diverticula.

'The Early Development of the Epiphysis and

Paraphysis in Amia,' by A. C. Eycleshymer and B. M. Davis. This paper describes and fully illustrates the early development of the same structures treated in the preceding article; each, therefore, supplements the other. The first vesicle of Hill (*i. e.*, the smaller more cephalic vesicle and the one termed 'secondary vesicle' by Eycleshymer and Davis) arises as an evagination from the dorsal wall of the other vesicle and some four or five days later.

It then shifts to the left side (occasionally to the right) and in one instance was seen to receive nerve fibers from the superior commissure. A critical review of the theories of the relation of the epiphysial outgrowths to the segmental sense organs follows.

The editor-in-chief gives a few aphorisms on 'The Ethics of Criticism,' which, though trite, are not, perhaps, wholly unnecessary.

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## *SOCIETIES AND ACADEMIES.*

### CHEMICAL SOCIETY OF WASHINGTON; THE 94TH REGULAR MEETING, MARCH 11.

THE first paper, 'Some Theories of Crystal Structure,' was by Mr. Wirt Tassin. After reviewing earlier hypothesis the author took up the more recent structural theory of Federow and Schonflies, which requires only that the structure of the crystal consist of similar molecules and allows the chemist and the physicist to decide the character of these molecules. It was pointed out that the essential difference between the Sohnckian hypothesis and that of Schonflies was that Sohncke requires that, in a system of points which is to have the characters required by a crystal structure, around every point the arrangement of the remainder is the same as around every other point; and all of his structures are derived by moving one point to another by sliding, rotating and screw motions. Schonflies, on the other hand, defines a crystal as 'consisting of absolutely similar molecules, so arranged that each molecule is environed in the same way by all the other molecules,' so that one part of the system may be derived from the other by reflection. Mention was made of Harlow's work and examples given, and the paper closed with a list of predictions of new compounds which have been verified and which were based upon theories treating of the relations between form, structure and composition.

As a supplement to Mr. Tassin's paper, Dr. F. K. Cameron read a short résumé on 'The Effect of Substitution on Isomorphism and Crystal Structure in Organic Compounds.' Substitution may cause the system to increase or diminish in symmetry or may cause the lengthening or shortening of axes. With the substitution of one or a few atoms by other atoms or groups the system generally changes and to one of less symmetry. If all, or nearly all, the equivalent atoms or groups are placed alike the derivative generally regains the symmetry of the original substance. The effect of substitution on crystal structure was illustrated by a comparison of the benzol and of the ammonium platino-chloride compounds. Neither (OH) nor ($NO_3$) seem to have much morphotropic value. Repeated substitution in the platino-chlorides often restores the symmetry of the mother substance, but the substitution of ethyl brings about a complete change.

Messrs. Wm. H. Krug and J. E. Blomen presented a paper, entitled 'A Recalculation of Wein's Table of Starch Equivalent to Copper Found Based on the Factor 0.92.' Starch or dextrine can be directly obtained from the copper found by converting the starch into dextrine and determining the latter with Allihn's solution. This table is based on the factor 0.90, which assumes that the formula of starch is $(C_6H_{10}O_5)n$ and that it is all converted into dextrose. Nägelli determined the formula of starch to be $C_{36}H_{62}O_{31}$ and if this is correct the factor becomes 0.918. Ost, working with the Sacchse method, decided upon the factor 0.925. In view of all these conflicting data Wiley recommends the factor 0.92, a man between the two last cited, which will give the analyst fairly accurate results. This factor has been used in the recalculation of the table.

The last paper, entitled 'Malt Wine,' was read by Dr. D. J. Kelly. He pointed out how Ordonneau, Jacquemin, Tettelin, Rommier and Sauer recognized the profound changes produced in the fermentation of a sweet liquid according to the kind of ferment employed. They found that when the juice of

a poorer variety of grape was fermented by the aid of the ferment peculiar to a choicer kind it became a wine having many of the characteristic properties of the better kind; a solution of sugar and water, to which appropriate yeast food and then wine ferment was added, was fermented and submitted to distillation; the result resembled brandy. When ordinary malt-wort is sterilized, and then, instead of being fermented with the usual saccharomyces cerevisiæ, or brewers' yeast, is fermented with the saccharomyces ellipsoideus, or fruit yeast, the resulting liquid resembles a fruit wine, and the liquid distilled therefrom a brandy. In his recently patented process Sauer ferments a sterilized malt-wort by a pure culture of a ferment derived from the bloom of the sherry or the Tokay grape, modifying the old process in certain important details, and obtains wine specimens of which were submitted to the audience and pronounced to be scarcely distinguishable, if at all, from the genuine articles. Dr. Kelly suggested that possibly the substitution of a properly selected grape or other wine yeast for that now employed in the process of bread making might be found to modify very agreeably this article of daily consumption.

V. K. CHESNUT,
*Secretary.*

## ZOOLOGICAL CLUB, UNIVERSITY OF CHICAGO—MEETING OF APRIL 14, 1897.

THE results of a study of the marine fauna of San Diego Bay, Cal., during January, February and March of the current year were presented. Specimens of most of the forms alluded to in the lecture were exhibited, among others *Renilla amethystina*, five new species of Polyclads, specimens of the Annelids *Phyllodoce*, *Polyophthalmus* and a huge *Amphitrite*, with its giant commensal *Polynoë*, specimens of several Crustaceans (two species of *Callianassa*, *Limnoria terebrans*, with wood showing its borings, *Pollicipes polymerus* and its egg masses, etc.). Several species of Polyplacophora, Opisthobranchs and specimens of *Octopus punctatus* were exhibited. Special attention was devoted to the Dicyemidæ, collected from the kidneys of over 100 *Octopus*. It was shown that three species of this peculiar group of parasites may be recognized on the Pacific coast, each belonging to a different genus and all new to science. There is a species of *Dicyema* to which the name *D. coluber* may be given and which, like its European congeners, has *four* metapolar cells in its calotte. A species of Professor Whitman' genus *Dicyemennea* and named *D. Whitmanii* has *five* metapolars. The third species belongs to a new genus which will be called *Dicyemodeca* (*D. sceptrum*, n. sp.). It has *six* metapolars an the parapolars are very short and broad, clasping the bases of the inflated metapolars like a collar. All the Pacific Dicyemidæ observed have orthotropal calottes. Some new facts concerning the sequence of the two peculiar forms of embryo during the life of the parent *Dicyema* were presented. In its youth *Dicyema* produces only the so-called vermiform embryos, but later only infusoriform embryos are developed within the axial cell. This is the reverse of the sequence formerly advocated by Professor Whitman, and would seem to indicate a relationship of the Dicyemidæ to certain Plathelminths; the series of vermiform embryos being perhaps comparable to the sporocyst and redia generations, the infusoriform embryo to the cercarian stage of the Trematodes. According to this view the adult of *Dicyema* is still to be found.

Of the Chordata several forms were exhibited —specimens of a huge red *Cynthia* and masses of *Ciona intestinalis* from the piles at Coronado, also several specimens of *Branchiostoma* (*Amphioxus*) *elongatum* (one measuring 90 mm.), dredged near the entrance to San Diego Bay. Considerable attention was paid to the viviparous Teleosts, more than forty species of which (Scorpænidæ and Embiotocidæ) occur at San Diego. These are all shore fishes and have become viviparous in adaptation to their littoral habitat. If the eggs were laid loosely in the water and allowed to float like the pelagic fish eggs they would readily drift ashore and perish; if attached to the rocks or bottom they would easily be destroyed by predaceous members of the very rich littoral fauna, unless guarded by the parent fish, as in the case of *Porichthys*, or hidden away under stones, as in the case of some of the Pacific shore fishes. When it is remembered that many of the viviparous Teleosts (especially the Embiotocidæ) live in

the surf on sandy beaches it will be seen that viviparity is a necessary condition of existence with these forms. A full series of the eggs and embryos of *Cymatogaster aggregatus* and the adults of this and five other species of Embiotocidæ were exhibited. Attention was also called to a number of specimens of the singular little blind fish (*Typhlogobius californiensis*, which lives in the burrows of *Callianassa* under rocks at Point Loma, near San Diego.

WILLIAM MORTON WHEELER.

NEW YORK ACADEMY OF SCIENCES—SUB-SECTION OF PSYCHOLOGY AND ANTHROPOLOGY, APRIL 26, 1897.

THE Academy met with Professor Thomas R. Price in the chair. The following papers were presented: 'Mental Imagery,' by Mr. W. Lay. The paper was a brief report of the result of two years' study and research. Of one hundred and twenty-five New York artists, the speaker found but three or four who exhibited the extraordinary degree of the power of visualizing which might be looked for in individuals trained to observe things from a purely pictorial standpoint. One hundred and fifty college students gave the same result. The speaker described the methods and gave the results of his experiments on himself to determine in terms of what sense the content of his own train of thought was chiefly composed. He has studied also the elements of mental imagery to be discovered in language and the visual, auditory and other imagery in poetry.

'Visual After-images,' by Mr. S. I. Franz. The speaker first described a typical after-image and referred to the interest the phenomena had aroused, as shown by the number of prominent scientists that had discussed them. Their importance was shown both for a correct theory of color vision and epistemologically as connecting links between sensation and memory and imagination. Experiments on the production (*i. e.*, the threshold) and on the duration were then described, and curves showing the results obtained were exhibited. The psychic relations of the different physical variables (viz. time, area and intensity) were discussed. The great individual variations, particularly in the coloration, showed that the after-image is not simple but exceedingly complex, and that the present theories to explain the phenomena are inadequate.

LIVINGSTON FARRAND,
*Secretary pro tem.*

THE ACADEMY OF SCIENCES OF ST. LOUIS.

AT the meeting of the Academy of Science of St. Louis, held on the evening of May 3, 1897, 21 persons present, Mr. H. Von Schrenk spoke of the respiration of plants, with special reference to the modification of those growing with their roots submerged in water. The lecture was illustrated by a demonstration of the liberation of carbon dioxide in respiration, from the roots of an ordinary flowering plant and freshly gathered fungi, and the more usual ærenchyma structures were made clear by the use of lantern slides.

Professor F. E. Nipher described a simple means of measuring the resistance of a tube to the flow of a current of air, when compared with an accepted standard, by the use of a tubular device similar in principle to the Wheatstone bridge, used in electrical instruments; the apparatus, in the present instance, consisting of parallel tubes filled with air, connected by a tubular bridge, in the middle of which a drop of water was placed, so as to change position with the variations in the flow of air on the one hand or on the other.

WILLIAM TRELEASE,
*Secretary.*

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*NEW BOOKS.*

*The Development of the Frog's Eggs; an Introduction to Experimental Embryology.* THOMAS HUNT MORGAN. New York and London, The Macmillan Company. 1897. Pp. x+192.

*Navahoe Legends, collected and translated* by WASHINGTON MATTHEWS. Boston and New York, Houghton, Mifflin & Co. 1897. Pp. viii+299.

*Introductory Course in Differential Equations.* D. A. MURRAY. New York, London and Bombay, Longmans, Green & Co. 1897. Pp. xv+234.

*The Science of Speech.* ALEXANDER MELVILLE BELL. Washington, D. C., The Volta Bureau. 1897. Pp. 56.